

A. Pitch

AN

ESSAY

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ON THE

HISTORY, CAUSES, AND TREATMENT

OF

TYPHUS FEVER:

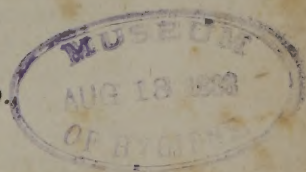
TO WHICH THE

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By ALFRED T. MAGILL, M. D.
Of Winchester, Virginia.



"Ex principiis nascitur probabilitas; ex factis vero veritas."

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ESSAY

ON

TYPHUS FEVER.

THE word Typhus, according to most writers, is derived from the Greek noun τυφος, stupor, a symptom which frequently supervenes, sooner or later, in the progress of the disease; oftener, however, coming on towards its termination, than existing in its earlier stages. The learned and scientific Good, however, refers the derivation of the term to the verb τυφο, which signifies "to smoulder," or "to burn and smoke without a vent," and thinks it "admirably expressive of the general nature of the fever to which it was applied at first, and which it designates at present; which burns not with open violence as the cauma, but with a sort of concealed and smothered flame."* There are, however, various other appellations by which this fever is known; such as the jail, camp, hospital, and ship fever. It has also been denominated the putrid, malignant fever, and from the frequent presence of petechiæ it is called, on the continent of Europe, febris petechialis, and constitutes the contagious fever of Lind, the febris inirritativa of Darwin, and the nervous fever of other writers.

The history of this fever is rather obscure. The ancient authors on medicine make no particular mention of it, from which we may conclude, that they were either unacquainted with it, or, considering it merely as a variety of the common fever of their respective countries, did not deem it worthy of

* Good's Study of Medicine, vol. 2, p. 129.

special notice. Sydenham was probably the first medical writer who described this disease, not, however, by the name under which it is now known. The account of it is contained in his "Essay on the rise of a new Fever." He says, "As soon as it began to thaw in February, 1685, the fever I intend to treat of here appeared, which I esteem a new sort of fever, and altogether unlike the fever of the eight foregoing years."* No one can read the symptoms of that fever as narrated by that great and good man, without remarking the striking similarity between them and the assemblage of symptoms which constitute the typhus of the present day; and, indeed, it has been considered as such by subsequent writers.† Sydenham does not mention that this fever was at all contagious, but attributed it to a particular state or constitution of the atmosphere.

In the year 1577, more than one hundred years anterior to the date of this fever described by Sydenham, a disease suddenly made its appearance, during the Black Assize at Oxford, in the month of July. This fever is generally cited as an instance of jail fever, and as affording incontestible evidence of its contagious character. The infection in this instance was supposed to have been communicated by one Rowland Jencks, a criminal, who "was arraigned and condemned in the presence of a great number of people to lose his ears. Judgment being passed, and the prisoner taken away, there arose such an infectious damp or breath among the people, that many there present, to the apprehension of most men were there smothered; and others so deeply infected that they lived not many hours after."‡ This fever is no doubt one of the instances alluded to by lord chancellor Bacon, when he says, that "The most pernicious infection, next to the plague, is the smell of the jail, where prisoners have been long, close, and nastily kept; whereof we have had, in our time, experience twice or thrice, when both the judges that sat upon the jail, and numbers of those who attended the business, or were present, sickened upon it and died."

Nearly nine years subsequent to this event, an infectious fever (as it was called) made its appearance at Exeter, during the assizes there, in March, 1586. This fever was attributed by many to contagion communicated by some Por-

* Wallis's Syd., vol. 2, p. 315.

† Wallis, in one of his notes to this essay, says, "Dr. Cullen considers this as a species of typhus, but of the milder kind, and arranges it under that genus by the name febris nova."

‡ Bancroft on Fevers, p. 430.

tuguese seamen, "who had been some time before taken at sea, coming with fish from Newfoundland, and "cast into the deep pit and stinking dungeon" of the "goale of the castle of Exon;" and "had no change of apparell," but being left to lie "upon the ground without succour or reliefe, were soon infected."* These persons, when brought into court for their trial, were supposed to have communicated the contagion to many of the individuals present.

"From that time (says Bancroft) I can discover no instance of any remarkable mortality or sickness, supposed to have been produced by jail infection, until the year 1730, (an interval of one hundred and fifty-three years,) when, at the Lent assizes, some prisoners, who had been removed from Ilchester jail, to take their trial at Taunton, were believed to have infected a part of the court, and produced a contagious disease"† of which many died.

Huxham, also, mentions in vol. 2, p. 82, of his work *de Aere, &c.*, that a disease, which he calls "*Febris putrida, contagiosa ac pestifera valde*," broke out at Launceton, in the month of April, 1742. He says, "*genita hoc in canceribus febris et per comitia provincialia disseminata longe lateque*."

In the year 1750, a fever made its appearance "during the sessions of the Old Bailey, which proved fatal to the lord mayor, and two of the judges, with several eminent and other persons, who, as was asserted, and is now generally believed, were infected by the contagion of jail fever brought into the court from Newgate."‡

Previous, however, to this occurrence at the Old Bailey, two essays had appeared on the subject, from the pen of Ebenezer Gilchrist, of Dumfries, which were published in the fourth and fifth volumes of the *Edinburgh Medical Essays and Observations*. He called it "nervous fever." Neither its origin nor its propagation was attributed by him to contagion.

A considerable time after this, Huxham and sir John Pringle published their essays on typhus—Huxham under the appellations of "slow nervous fever" (*febris lenta nervosa*) and "malignant putrid fever,"§ and Pringle under that of

* Bancroft, p. 434.

† Ibid, p. 110.

‡ Ibid, p. 111.

§ The "slow nervous fever" and the "malignant putrid fever" of Huxham, are obviously only different *grades* of the same disease; they correspond to the typhus mitior and gravior of nosologists.

"jail or hospital fever." Both these writers were strenuous advocates for the attribute of contagion as belonging to this fever. Subsequent to these publications we find much more attention paid to this subject, and its pathology and causes have been investigated with much ardor and diligence. Many of those who have written on it have contended for its being contagious; some there are, however, who dissent from this doctrine, and deny that it possesses any such property.

This fever has been divided by nosologists into the *typhus mitior*, (mild typhus or nervous fever,) and *typhus gravior*, (malignant or putrid typhus;) thus indicating its sometimes mild and sometimes violent character. This distinction appears to us to be entirely useless. It produces an unnecessary multiplication of words; the same language descriptive of one grade being applicable, with a slight variation, to the other; so much so, that both can be easily comprehended in one general description, and it, moreover, leads to no good practical result. The typhus mitior and gravior being only grades of the same disease, are to be treated exactly on the same principles; and that physician who has not sagacity enough to discriminate between a mild and a severe fever, will not have his intellectual vision brightened by such distinctions. Independent, however, of these reasons, we do not think, in practice at least, that the distinction can always be made. However easily it may be done in the closet, it will be found often impossible at the bedside of the patient. Every physician of much experience in this disease, will acknowledge that, in a majority of cases, the symptoms are such as to render it doubtful to which grade they belong. Wilson remarks, that "between the worst and mildest forms, there are innumerable degrees which insensibly run into each other;" thus rendering it impossible to draw a distinct line of demarcation between them. Those, therefore, who are in favor of distinguishing typhus into grades, should carry out this principle, and give a separate description of each of its "innumerable degrees;" for there would be just as much ground for this, in a practical point of view, as there is for the other; they should also extend this principle to other fevers, for if this distinction be necessary in typhus, it is equally so, for instance, in yellow fever, for it also has its gradations of violence and mildness. Another objection to it is this: the different grades of this fever are so readily convertible into each other, that, according to the treatment pursued, what was mitior today, may be gravior tomorrow, and *vice versa*. For these reasons we reject these distinctions, which we consider arbitrary and useless.

Armstrong, in his unequalled work on typhus fever,* notices three varieties instead of two. He divides the disease into the typhus of simple excitement, the inflammatory and the congestive typhus; though we have the same objection to his two first divisions, that we have to those of mitior and gravior, yet, nevertheless, we think his arrangement on the whole better than the other; the phraseology which he employs to designate the different forms of this disease is, we think, preferable to that of mitior and gravior, inasmuch as it expresses in itself the real character and pathology of the disease, and strikes at the root of those notions of debility and putrescency as prominent features of this fever, which we derive from books, and which are intimately associated with all our reflections on it; and it moreover includes a form of the disease, which unquestionably demands a separate consideration, i. e. congestive fever. Until the publication of his work, we had no distinct conception of the pathology of congestive fever; his post mortem examinations, however, have revealed its true character; and his graphic pen has delineated with so much accuracy its symptoms, that it cannot fail to be recognized by even the most careless observer. But though we prefer his arrangement to the other, we shall adopt it only in part. We will include his two first divisions in one general description, and then treat of congestive typhus, under a separate head. We will now proceed to an enumeration of the symptoms of typhus.

Symptoms. This fever commences with the same symptoms which usher in the attack of other febrile affections.† A feeling of languor and an indescribable sense of weariness and dejection, are usually the first links in the chain of symptoms which constitute this disease; there is also, usually, some slight gastric distress, sometimes amounting to nausea, together with a total loss of appetite. There is, in some cases, a cold creeping sensation over the body; in others, particularly when the attack is severe, this coldness amounts

* We cannot mention this work, without expressing the unbounded admiration we feel for the talents of him who composed it, and our conviction of its great value. If we were required to put our finger upon a volume, containing the greatest amount of practical information on the diseases of which it treats, we should unhesitatingly select it in preference to any work with which we are acquainted. The neatness, elegance, and perspicuity of its style, the vast fund of practical instruction spread over its pages, and the vital principle of genius which pervades every portion of it, has always rendered its perusal, to us, an intellectual treat.

† Pringle remarks, that "the disease in the beginning is not easily distinguished from any common fever."

to rigor, alternating with quick and transient flushes of heat; the face is pale, with some slight appearance of livor under the eyes; and there is more or less of a load, pain, or giddiness of the head—the pulse at this time is generally weak, low, and frequently irregular. This assemblage of symptoms constitutes what has been aptly termed by Armstrong the stage of oppression. This may continue from a few hours to as many days; sooner or later, however, evidences of decided reaction supervene. The face loses its paleness and generally becomes much flushed; the pulse increases in frequency; in some cases it is tense and hard; the headache becomes excruciating, and the mind is often overwhelmed with delirium. The skin grows very warm, and the heat appears to have a penetrating quality.* The thirst becomes considerable, and, in some cases, almost unquenchable. In milder cases these symptoms are much less prominent and severe: in such cases, the pulse, though quicker than natural, is neither hard nor strong, and the uneasiness of the head not near as great; the delirium, if present, is seldom violent and uncontrollable as in the severer modifications of the disease. Petechiæ also frequently make their appearance, though they are by no means peculiar to typhus, but frequently occur in other fevers; the mouth is parched and dry, and the tongue, from the beginning, is more or less furred, and, in the progress of the disease, generally brown, and sometimes black; the bowels are often much disposed to constipation, and the secretions and excretions are much charged; the alvine discharges becoming dark and offensive, and the odor of the breath and body very unpleasant. After a longer or shorter time, varying from the seventh to the eighteenth day, and, in severe cases, sometimes a much shorter period, a change takes place in the character of the disease; what Armstrong calls the stage of collapse supervenes, and if the patient has been properly treated during the period of excitement, it is usually the beginning of a return to health, which is indicated by an abatement of all the

* Pringle notices this symptom. "On first touching the skin," says he, "the heat seems inconsiderable; but upon feeling the pulse for some time, I have been sensible of an uncommon ardor, leaving an unpleasant sensation on my fingers for a few minutes after. The first time I observed this, I referred it to the force of imagination, but was assured of the reality by repeated experiments, and by the testimony of others, who, without knowing of my observation, had made the same remark." Other physicians have also mentioned it. Frank, in allusion to it, says, "*sæpe manifeste acer digitosque urens.*" It has been called by others a biting heat. It is not, however, peculiar to typhus, as we have observed it in other fevers.

symptoms; but if (particularly in severe cases) the fever and topical inflammation, which are almost always present in such cases, have not been kept in proper subjection, it is a change pregnant with the utmost danger to the patient; the delirium is lost in profound stupor, the tongue becomes exceedingly black and dry; dark sordes collect about the teeth; subsultus tendinum, and what have been denominated symptoms of putrescency, appear; the petechiæ, if present, turn to a dark purple color; hemorrhages from the stomach, bowels, and kidneys, (the last making the urine dark,*) come on, or "profuse ichorous and involuntary stools" soon extinguish the remains of life. The patient, however, is sometimes carried off by convulsions.

Prognosis. In forming our prognosis of the probable event of this fever, we must not deduce our opinion from any one symptom, however prominent it may be, but rather from mature deliberation upon the symptoms taken collectively: as the chief danger, not only in this, but in all fevers, arises from the disorganizing effects of high and uncontrolled excitement, upon some one of the vital organs, it follows, that if this be moderate, or if it be even high, and we find that we can keep it in due subjection, then we may rationally anticipate a favorable termination; but if neither of these conditions obtain, then we may justly dread the event. But to be more particular: we may calculate on recovery when the delirium is slight, the strength not much impaired, the tongue moist and beginning to clear, bowels loose and easily operated on by medicine; when deglutition is performed without difficulty, and the respiration is free and easy,† with a soft and moist skin. A deafness coming on during the fever is considered by many a favorable sign.‡

* Huxham says, "I have frequently seen the urine, in petechial fever, almost black."

† Fordyce on Fever, page 221, observes, that, "although the patient should be insensible to all external objects, although he should sleep very little, yet, if the *deglutition and respiration* should remain unimpaired, the patient is not to be despaired of; it happens even that he most commonly recovers. But if he *respires with great difficulty*, or hardly at all, or if the deglutition be almost totally prevented, or if the attempt to swallow throws him into convulsive contractions, he rarely recovers."

‡ We have mentioned deafness as one of the favorable symptoms in typhus, more upon the authority of others, than on our own conviction of its being such; to us it clearly indicates a degree of fulness of the cerebral vessels, which, in our opinion, is much to be deprecated. That it is owing to this cause is plain from its being removed by evacuants, and by its returning if these are discontinued. Every physician must have witnessed this. Hippocrates testifies to this fact. In section 4th, aphorism 28th, he

Besides the signs already mentioned, there are what are called critical discharges. Among the most frequent of these, is sweating; this evacuation was at one time considered of so much importance in the cure of fever, that it was thought a *sine qua non* to recovery,* and accordingly the most heating and pernicious means were employed to force this discharge. This destructive and irrational practice is fortunately almost exploded at the present time, and physicians now, with the exception of using a few antimonials, generally leave it to nature, who effects it best in her own way.†

The most frequent critical discharge is a diarrhœa; this, when of a proper kind, very often terminates the fever; it is always most beneficial when the stools are bilious and somewhat consistent; profuse liquid and colliquative discharges always do harm, and often irretrievably prostrate the patient in a very short time. This last description of diarrhœa is, of course, to be immediately arrested; the first kind ought rather to be encouraged. Hemorrhages, likewise, often prove critical in this fever; the blood may proceed from the nose, stomach, or bowels. Persons have been often rescued from the grave, when given up as lost, by the opportune occurrence of hemorrhage from the bowels, and other parts; they should always admonish the practitioner to relieve nature from the necessity of resorting to this hazardous alternative, by using proper depletion in the first stages of the disease; we say hazardous alternative, because it often comes too late, and as often is excessive in quantity. Hemorrhage only does good in typhus when it occurs early in the disease, before the constitution has been shattered by the long continuance of the fever. This fact did not escape the sagacious mind of the father of medicine. In one of his

remarks, "*Quibus biliosæ sunt egestionēs, surditate superveniente cessant; et, quibus surditas est, biliosis supervenientibus cessat.*" Although, in the first part of this aphorism, he seems to consider the cessation of the bilious discharges as the effect of the deafness, yet it is obvious from the latter part of it, in which he says this deafness is removed by the recurrence of these evacuations, that he had mistaken the cause for the effect.

* Huxham declares that "he never once saw a malignant typhus cured, till more or less sweat had issued."

† To Sydenham the praise is justly due for having put down the pernicious practice of endeavoring to force a perspiration, by the use of the most heating and inflammatory measures, and of substituting in its place the cooling and evacuating plan of treatment. It constituted by no means the least of his triumphs over the deep rooted prejudices of the times in which he lived.

aphorisms, he says, that "livid, bloody, fœtid, and bilious stools, coming on in continued fevers, are alike unfavorable; but a timely appearance of them beneficial."*

This fever also terminates occasionally in glandular swellings. Dr. Donald Morris, in his work on diseases of the army, observes, that "swellings of the parotid glands appeared in many subjects towards the decline of the fever, which came to suppuration and proved critical." Pringle, and other writers, also mention their occurrence, and recommend an early incision to discharge the pus.

Having enumerated the favorable symptoms, we will now briefly mention those which portend the approach of death. The bad signs are high delirium or deep stupor, subsultus tendinum, black, dry, and tremulous tongue, and inability to protrude it beyond the lips; difficult deglutition, and impeded respiration; weak, irregular, and intermitting pulse; great restlessness, evinced by frequent attempts to get out of bed, or by continual tossing from one side of it to another; the surface of the body bedewed with a cold sweat; frequent colliquative stools and hemorrhage from the mouth, ears, &c.; sometimes the patient expires in convulsions. Such is the dark catalogue of symptoms which declare death to be at hand. Even these, however, should not deter us from exertions to save life; persons sometimes recover with nearly all the above mentioned desperate symptoms, and the knowledge of this fact should teach us the important lesson never to abandon, as entirely hopeless, any case of fever; appearances are sometimes fallacious,† and we should never fold our arms in inactivity, though a combination of the worst symptoms exist.

Pathology of Typhus. Typhus fever has been for a long time, and still is with too many, associated with the ideas of debility and putrescency as its standing and prominent symptoms. The prejudices on this head, however, are beginning to lose some portion of their sway, in consequence of the flood of light which has been shed on the pathology and treatment of typhus, by some recent publications. Among these the work by Armstrong stands conspicuous; the appearance of which may be said to have constituted a new era in the history of typhus. But while bestowing due praise on

* "Excretiones in febribus non intermittentibus lividæ, et cruentæ, et graveolentes, et biliosæ, omnes malæ sunt. At probe secedentes bonæ."

† Hippocrates remarks, that "acutorum morborum incertæ admodum, ac fallaces sunt prædictiones."

this distinguished writer, let us not forget the illustrious Sydenham, whose sagacious and strong mind, more than a century and a half ago, discerned the true character of this fever, and suggested a course of practice, which, though in a great measure lost or forgotten amid the conflicting opinions of subsequent writers, has been revived in the present century, and is now, with but little variation, followed by all those who do not still hold to the doctrine of debility as belonging to this disease in all its stages. But to return to the subject. We will endeavor to show, from the various circumstances and phenomena incident to it, that, so far from being a disease of debility, it is essentially one of excitement, and frequently of inflammation as its consequence. One form of typhus, (i. e. the congestive,) will not, of course, be comprehended in this.

A state of real debility is a condition of the system which is, generally, (we might say always,) the consequence of long continued excitement, or the more gradual, though not less certain, result of an insidious chronic affection, preying upon some important and vital viscus. This *real debility* is always increased by such remedies as are termed evacuants, and, on the contrary, is diminished by such means as add tone to the system; but if, in the commencement of a fever, a person appears very weak, and if we find that the administration of tonics and stimulants only increases the debility, and, on the other hand, that evacuants, such as bleeding and purging, lessen it, the legitimate conclusion from these facts is, that this weakness is not real, but only apparent. This, then, is the case with the debility in the primary stages of typhus; it is increased by the exhibiting of stimulants and lessened by evacuating measures. Armstrong, whose experience was very great in this fever, bears testimony to this fact: "In the first stages of typhus," says he, "the debility is only apparent, and chiefly dependent upon the preternatural accumulations of blood in the veins about the head, heart, liver, and other internal parts, while there is less circulating upon the external surface of the body, than in a natural state. In the second stage, the debility is still only apparent, being then the consequence of over excitement of the heart and arteries; but in the third and last stage, beyond all dispute, the debility is real, as it is then connected with a general collapse, which sooner or later succeeds to a state of febrile excitement, as certainly as exhaustion follows a fit of intoxication." And a little farther on he says, that though "this proposition is not evident at first sight, yet it is as certainly true as any in physic, and is proved from the debility in the

first and second stages being increased by wine and cordials and lessened by a spare regimen and evacuations.”*

Every physician must have witnessed the positively strengthening effects of cathartics in typhus fevers, before the constitution has been really debilitated by long continued excitement. A patient who has been carried to the close stool, after a copious evacuation from his bowels, is able to get up, and walk without assistance to his bed.† This invigorating effect of purgatives in typhus and other fevers, is of frequent occurrence, and is totally unaccountable upon the supposition of its being a disease of debility; we commit, therefore, an egregious error when we mistake this apparent for real debility. In the strong language of Rush, “the clown who supposes the crooked appearance of a stick, when thrust into a pail of water, to be real, does not err more against the light, than that physician errs against a law of the animal economy, who mistakes the debility which arises from oppression for an exhausted state of the system, and attempts to remove it by stimulating means.” But again, if this disease be one of great debility, there are many of its phenomena, mentioned by those who advocate this very theory, totally inexplicable, and indeed irreconcilable with this view of the disease. We presume every one will admit, that, if real debility be a characteristic of this fever, what are obviously debilitating discharges must increase this symptom, and of course prove prejudicial to the patient; but that this is not the fact, we will endeavor to prove from the testimony of others. Wilson observes, that a “spontaneous flow of blood from different parts of the body, we have seen, is frequently attended with an abatement, or total removal, of the symptoms of synocha, and, what we should not *a priori* have expected, sometimes those of typhus;”‡ and in another place, when speaking of typhus, he says, “I have often seen them (i. e. hemorrhages) attend a general abatement of the symptoms.”|| We would ask Dr. Wilson how he can reconcile these facts with his notions of typhus fever?

* Armstrong on Typhus, p. 25.

† This is also frequently observed in other fevers. Dr. Bryce, in his account of the yellow fever on board the Busbridge Indiaman, in 1792, observes, that he has “often seen a man carried upon deck, perfectly delirious with subsultus tendinum, and in a state of the greatest apparent debility, who, after one or two copious dark colored evacuations, has returned of himself, and astonished at his newly acquired strength.”

‡ Wilson on Fever, vol. 1, p. 142.

|| Ibid. p. 134.

The good effects of long continued and profuse sweating in this fever cannot be accounted for on the supposition of typhus being a disease of debility. We can well imagine that the loss of quarts of fluid might be salutary in a disease of high excitement, but it could never enter into our conception that the same loss would be beneficial to one who was really and *bona fide* weak; and yet writers mention the good effects of several days' continuance of profuse perspiration.* A diarrhœa, also, as was mentioned before, is often critical in this fever. We have thus endeavored to prove, from the very character of the debility in the first stages of typhus, and from the phenomena and circumstances incident to it, that the opinion of those who consider it a disease of genuine debility, and, as such, requiring tonic and stimulating remedies for its cure, is erroneous.

We will now take the opposite side of the question, and will endeavor to prove that it is a disease of excitement, and often of inflammation as its consequence. In the first place we will observe, that all the circumstances and phenomena of typhus already mentioned, and which are inexplicable upon the ground of its being a disease of debility, are easily accounted for upon the supposition of its being a disease of excitement and often of inflammation. The opposite effects of stimulants and evacuants in the first stages of typhus, the relief afforded by hemorrhages, profuse and long continued sweating, diarrhœa, and the abundant and free discharge of matter from glandular swellings, are perfectly consonant to, and exactly what we would expect from, this view of the disease. But we have still better ground for this opinion: post mortem examinations prove it to be correct; they establish, beyond doubt, that death in this fever is generally produced by the destructive ravages of inflammation on some one organ or organs of our frame. Many of the writers on this fever have recorded the results of their dissections. Pringle relates the appearances, on dissection, of ten persons who died of jail fever, in all of which, either inflammation or its effects were plainly visible; a majority of them had suppuration of the brain. Thomas says, "The usual appearances on dissection are, a softness and flaccidity in the solids; a dissolved state of the fluids, particularly of the blood; collections of sanious matter in the different cavities; turgescence and inflammation of the thoracic and abdominal

* Morris, in his treatise on diseases of the army, remarks, that the sweating often continued, with the best effects, for several days. Hoffman, also, makes a similar statement.

viscera; and in the interior parts of the brain increased vascularity and collections of serous fluid.”* Armstrong mentions that his post mortem examinations always exhibited the appearances or consequences of inflammation. Dr. Dickson, in his account of the contagious fever which prevailed in the Russian fleet, observes, “that the appearances on dissection were strongly illustrative of the frequency of visceral inflammation, in the fever which he saw and described; for the brain, lungs, and contents of the abdomen were severally found to have been attacked, among those who were the victims of this disease.”† Ferriar also mentions the appearance on dissection of several persons who had died of typhus, all exhibiting the most unequivocal evidences of inflammation. These dissections seem to have made such an impression on him that he observes, after having mentioned the results of them, that “perhaps local inflammation is more frequently joined with typhus than we are aware.”‡

Other accounts of post mortem examinations, corroborative of the above, might be mentioned, but these are deemed sufficient to establish, beyond doubt, the pathology of typhus, and that death in this fever is generally, if not always, the result of inflammation, disorganizing some important part of the animal machine. So universally, indeed, have the traces of inflammation been observed in those who die of this and other fevers, that many physicians have adduced it as the cause of fever. Clutterbuck, Ploucquet, Mills, Marcus, Reid,|| and Broussais, are the great advocates for the local origin of fevers. The latter, especially, a physician of eminent talents and much experience, has gained great celebrity by the ingenuity and ability with which he sustains his doctrine, and by the imposing array of talent which has flocked to his standard. His doctrine is, that fever arises from an inflammation of the mucous lining of the stomach and bowels.

It would be irrelevant to the object of this essay to attempt any thing like a labored refutation of this theory; which, en-

* Thomas's Practice, p. 49.

† Armstrong on Typhus, p. 154.

‡ Medical Histories, p. 76.

|| Clutterbuck and Mills locate the inflammation, which they consider the cause of fever, in the brain. Ploucquet contended that fever was sympathetic of an inflammation of the brain, stomach, and intestines; and Reid considered its seat to be the spine. Professor Marcus, of Bavaria, taking a more enlarged view of the subject, regarded all inflamed organs as equal causes of fever.

forced with all the "zeal, energy, and talents which this celebrated school can bring into the field," has spread itself extensively over the continent of Europe, and, having crossed the waters of the Atlantic, can number among its supporters many of the distinguished medical characters of this country. We are not among those, however, who assent to the truth of this doctrine; on the contrary, we reject it unqualifiedly, as being, in our opinion, utterly at war with the true pathology and phenomena of fever. We do not believe fever arises from inflammation of the mucous membrane of the stomach for the following reasons. 1st. Because in many cases we have no evidence of its existence at the commencement of fever, or for even several days after; and not unfrequently during the whole course of the disease, we have no one symptom indicative of its presence. 2d. If inflammation of the mucous membrane of the stomach and bowels be the cause of fever, then we should always have, what but rarely exists, some degree of tenderness of the epigastrium, and a constant mucous diarrhœa; the latter symptom, particularly, should be present in every case of fever; for it may be laid down as an almost invariable rule, that a part which in a healthy state secretes a fluid, has that secretion increased when it becomes inflamed; this effect we see produced in the mucous membrane of the nose, larynx, and trachea in catarrh, for example, which consists in an inflammation of these parts; and to cite a case still more in point, we find it to be strikingly the case in dysentery. All acknowledge that the mucous membrane of the intestines is inflamed in this disease, and we see increased secretion of mucus, the result of it; the same thing happens when the lining membrane of the bladder is inflamed; as we have not, therefore, this discharge in typhus, in which inflammation of the mucous membrane of the stomach and bowels is assumed to exist, we conclude that it is not present. 3d. We cannot conceive inflammation to exist in the first stage of fever; when the action of the heart and arteries is comparatively weak and feeble, and the blood, as a necessary consequence of this weakened action, accumulates in the *venæ cavæ* and their branches. 4th. This very accumulation of blood about the heart, inasmuch as it affords to it an increased quantity of its natural and legitimate stimulus, is abundantly sufficient to produce a reaction of this organ, or, in other words, to excite a fever, without bringing in a cause (i. e. inflammation of the stomach and bowels) which we cannot conceive to exist under such circumstances. And, 5th. Post mortem examinations do not prove it; they may, indeed, exhibit the evidences of inflam-

mation in the lining membrane of the stomach and bowels, in the head, liver, lungs, spine, &c.; but this by no means proves, that the inflammation which is found to have existed in these different parts was antecedent to and produced the fever; and if we admit inflammation as a cause of fever, when found in the mucous membrane of the stomach and bowels after death, we must make the same admission when this inflammation is discovered to have existed in other parts; and, as a consequence of this, we should have as many different seats of fever as there are viscera. For these reasons we reject the doctrine of Broussais and of all others who contend that fever has "a local habitation;" and in doing so we are sustained by the opinion of the most eminent pathologists of the present day. Laennec affirms, that the organic changes which we find after death from fever, are "*evidemment posterieures a la fièvre, et ne peuvent, par conséquent, être regardées comme sa cause.*"*

The views of those who would limit the seat of fever to some one viscus are very unphilosophical; "they narrow an important subject which disdains such petty limits." Fever is what Dr. Hosack, one of the most distinguished physicians of this country, describes it to be, "a disease of the whole system; it appears no less in all the faculties of the mind than in all the functions of the body; it shows itself in every organ of our frame, and affects every nerve and fibre of the system; the absorbing, the circulating, and excreting system of vessels, are all affected by it; in a word, it is omnipresent; it has no one pathognomonic symptom, but is constituted by a concurrence of symptoms, and these variously combined in the various forms that fever assumes, depending upon the cause whence it proceeds, and the condition of the body in which it occurs."†

In rejecting the doctrine of Broussais as to the local origin of fever, we entirely concur with Armstrong in the opinion, that inflammation is the product of preceding excitement, and that its particular seat is determined by the previous condition of the organ which it attacks; this condition is a debility of the organ affected.‡ From what we have seen we think the head is most frequently affected.

* Med. Chir. Review, Jan. 1825, p. 16.

† Appendix to Thomas's Practice, p. 948.

‡ Ferriar long ago advanced this opinion; he says, that "during the increased action of the circulating system, if any part of the body be originally weak, or have been rendered infirm and irritable by preceding disease, congestion and its consequence may be expected there." Med. His. p. 150.

Congestive Typhus. Previous to the appearance of Dr. Armstrong's admirable work on typhus fever, this form of the disease had not attracted the attention it merited from physicians; and, in fact, we knew little or nothing of its nature. In the works of Sydenham we find the first particular notice of congestive fever; he relates the circumstances of one case only, but from the confident tone in which he spoke of what would be the effect of venesection in it, we may fairly presume that he had met with cases of the like description before. In allusion to this particular form of the fever, he remarks, "that all the symptoms proceed from nature's being in a manner oppressed and overcome by the first attack of the disease, so as not to be able to raise regular symptoms adequate to the violence of the fever; all the appearances being quite irregular. For, the animal economy being disordered, and in a manner destroyed, the fever is thereby depressed, which in the true natural order generally runs high. I remember (continues he) to have met with a remarkable instance of this, several years ago, in a young man I then attended; for though he seemed in a manner expiring, yet the outward parts felt so cool, that I could not persuade the attendants he had a fever, which could not disengage and show itself clearly, because the vessels were so full as to obstruct the motion of the blood."* After the time of Sydenham, no particular notice appears to have been taken of congestive fever. It is true, that Drs. Jackson and Clark, almost a century after, gave some imperfect account of it; but until the appearance of Armstrong's work, our notions of the nature of this fever were very obscure; he has the high merit of having clearly and ably elucidated its pathology; and of having opened a new field for medical enquiry and research. The disease, according to him, consists in an engorgement or distension of the large internal veins; and differs from his other divisions of typhus in having no stage of excitement or reaction of the heart from the first shock of the disease. We cannot do better than copy his description of the symptoms of congestion; they are portrayed in his best style. "The attacks (says he) of the most dangerous forms of the congestive typhus are generally sudden, and marked by many remarkable symptoms: an overpowering lassitude; feebleness of the lower limbs; deep pain, giddiness, a sense of weight in the encephalon; a dingy pallidness of the face; anxious breathing; damp, relaxed, or

* Wallis's Sydenham. vol. 2. p. 351.

dry withered skin; and those peculiar conditions of the temperature which have been noticed above.* The pulse is low, struggling, and variable; the stomach irritable; frequently there is an inability from the first to hold up the head; and the mind is more often affected with dulness, apprehension, and confusion, than with delirium. The whole appearance of the sick impresses the attentive practitioner with the idea, that the system in general, and the brain in particular, are oppressed by some extraordinary load. Both the manner and look of the patients undergo early and great alterations; sometimes they slowly drawl out their words, or utter them in a hasty and yet imperfect mode, like people who slightly stammer when embarrassed; they not unfrequently seem as if stunned by a blow, half drunk, or lost in a reverie; and at times have the bewildered aspect of persons suffering under the first shock of an overwhelming misfortune. The eye is occasionally glassy and vacant, without redness; but at other times it is heavy, watery, and streaked with blood, as if from intoxication or want of sleep. At the commencement the pulse is often less altered as to frequency than might reasonably be expected, yet in general it becomes very rapid towards the close; the tongue is usually little altered in the first stage; but in the last, it is frequently rough, foul, and brown; the bowels are mostly very torpid in the beginning, and the stools procured dark and scanty; whereas, in the last stage, the bowels are generally loose, and the stools copious and involuntary. Eructations are not uncommon at all times, and the epigastric region is often much inflated. On account of the general torpor, the secretions are diminished or suppressed; and, as justly remarked by Dr. Robert Jackson, the skin is often in that peculiar state, that if blisters be applied, they either do not act at all, or so defectively, as to leave the appearance as if the part had been seared by a heated iron. Petechiæ in general appear earlier in these than in any other varieties of typhus; and in the last stage there are sometimes gangrenous spots on the extremities, oozings of blood from the mouth and nostrils, and hemorrhage from the bowels.”†

“There are comparatively milder forms of congestive typhus, in some of which the patient walks about for a few days after the infection has begun to operate, and complains little, except of uneasiness of the head, loss of appetite, and languor, appearing rather paler than when in health. If

* “Shrivelled cool skin.”
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† Armstrong on Typhus, p. 74.
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strictly attended to, however, by a medical observer, a change may usually be remarked in his whole demeanor; he cannot as steadily command his attention as before, is not only restless during the day, but watchful at nights, and soon betrays an absence of mind or loss of memory. At length he becomes garrulous like a half drunken person, or talks inconsistently with his former views and character; after the lapse of a day or two the mental confusion is most obvious to every one—he begins to be unsteady in his gait, and has a heavy intoxicated cast of countenance. If carefully examined at this period, his tongue will be found white, his pulse small, quick, and perhaps irregular; his breathing hurried; his bowels slow; his skin rather hot about the trunk, but coolish and damp on the extremities. If the disease be allowed to proceed, without decided interruption, the hands shortly become tremulous, and the confusion of mind passes into delirium; yet there is still a want of regular excitement, demonstrated by alternate flushing and paleness of the face, the feebleness of the pulse, the unequal state of the whole circulation, the coolness of the extremities, the partially concentrated heat of the trunk, and laxity of the skin. Aural and visual deceptions succeed, and force the patient into violent exertions, and every attempt to overpower him by coercion tends to aggravate the delirium and sink the strength. His tongue grows daily fouler and his debility greater; he begins to pick the bed clothes; and, at last, petechiæ and subsultus tendinum appear. About this period, the general turbulence sometimes unexpectedly abates, and he may become as serene and rational, as to give hopes that a favorable crisis has taken place; but the calm is most frequently deceitful, being soon followed by a universal collapse, in which death occurs, mostly without much struggling. Several cases, nearly answering to the above description, have fallen under my notice, and I have found, that if opportunely and properly encountered, they may generally be subdued; but that if overlooked or improperly treated in the commencement, they will commonly baffle the best directed measures.”* There are some other modifications of congestive typhus, which it is deemed unnecessary to take up time and paper to relate, because from what has been said the disease may be easily detected in all its different shapes.

Prognosis. The prognosis in congestive typhus, is to be drawn from the character of all the symptoms present in the

* Armstrong on Typhus, p. 75.

disease. If these should become worse, if the delirium should increase, the pulse should continue to sink, the breathing to grow worse, the skin become colder, and the stools be passed involuntarily, it will generally terminate fatally. If, on the contrary, the reverse of this should take place, then we may anticipate a favorable termination.

Pathology. The nature of this fever obviously consists in an accumulation of venous blood in the interior of the body. The cause of this fever, whatever it is, acting either directly or indirectly on the heart,* produces weakened action of this organ, and this latter necessarily causes an accumulation of blood in the *venæ cavæ* and their branches; less blood than usual being sent to the surface, paleness and shrinking of the features and surface of the body is the necessary consequence. This state of the circulation, in a greater or less degree, is common to the first stage of all fevers; the heart, however, usually reacts under the pressure of venous blood, and drives it with increased force to the exterior, when the fulness of the features and the warmth and color of the skin return; but if, from any cause, the heart should fail in re-

* When reflecting on the *modus operandi* of the remote cause of fever, we have often been astonished, that all writers on the subject should have fallen into the error of overlooking its effect upon the action of the heart; and to have confined their observation to the effect (secondary and consecutive) produced on the cutaneous vessels. Thus Armstrong, in his essay on Scarlet Fever, says, "One of the most obvious operations of contagion, like that of dolo, is a change of action in the cutaneous vessels, and a recoil of blood from the surface towards the centre." Now, it is perfectly obvious that the action of the cutaneous vessels is dependent on that of the heart; if the action of the latter is weak, so likewise will be that of the former—if strong or vigorous, so will be that of the cutaneous vessels. So long, therefore, as the heart preserves its action unimpaired, the cutaneous vessels must continue in the same state; but the moment that this is weakened, we then observe this "change of action in the cutaneous vessels," which Armstrong considers to be the primary and direct effect of the operation of contagion, and the first link in the series of effects which constitutes fever. In consequence of this weakened action of the heart, less than the usual quantity of blood is sent to the surface, and as the skin is generally flushed, and the features full and expanded, when this action is exalted and vigorous, in fever for example, so likewise when it is weakened we have paleness of the face and shrinking of the features and surface of the body. The phraseology of Armstrong is incorrect and calculated to mislead, when he says that contagion produces "a recoil of blood from the surface towards the centre;" for it would seem from this that the cause of fever impressed upon the contents of the cutaneous vessels a centripetal direction, which is palpably absurd. The reader will find this subject most ably discussed in a work on the practice of medicine by John E. Cooke, M. D., Professor of the Practice of Medicine, in the University of Transylvania, a book containing more sound and original views, and a more correct pathology, than any with which we are acquainted.

covering its energy, then we have a continuance of those symptoms which characterize congestive fever. Whether, however, the cause (i. e. weakened action of the heart) assigned for this accumulation be the correct one or not, dissections demonstrate beyond doubt its existence. Armstrong says, that "In examining the bodies of some patients who had died in the most concentrated attacks of congestive fever, I have found the right side of the heart loaded with dark blood; and, in reflecting upon the phenomena of all, am now inclined to believe, that their pathology is intimately concerned with the functions of the right ventricle. For (continues he) when the action of the right ventricle is diminished, and when it is overloaded by too great an accumulation of blood, it must, by consequence, occasion a remora of venous blood in distant organs; and a sufficient portion of red blood not being thereby returned to the left ventricle, its action, also, must be defective, and its blood not sufficiently oxygenized for the complete purposes of vitality."* The whole of this extract is highly valuable and important, as establishing by post mortem examination the pathology of this form of fever; but a part of it, we think, deserves particular attention, inasmuch as it may serve to explain the cause of the sudden deaths which frequently take place during the prevalence of mortal epidemics. I allude to that part in which he says, that "an accumulation of blood about the heart must produce a remora of venous blood in distant organs." Or, in other words, if the right side of the heart be full and distended with blood, this fulness must extend itself into the cavæ and their branches; following, therefore, the ramifications of the superior cavæ, we find this fulness and distension extending up the jugulars, from thence through the foramen lacerum into the larger sinuses of the dura mater; when, therefore, this accumulation is already very considerable, if, from any cause, it should suddenly increase, it may produce such pressure on the brain as to cause the person to drop down as if in apoplexy. Armstrong, indeed, says, that "the most violent forms of the congestive typhus, resemble apoplexy in their symptoms, to which, indeed, they often have a near affinity in their pathology."† This is the best explanation we can offer of those sudden and occasionally instantaneous deaths which sometimes occur during the raging of violent fevers.

Treatment of Typhus Fever. As our opinion of the character and pathology of typhus fever differs as entirely from that of those who consider it a disease of debility in all

* Essay on Typhus, p. 84.

† Ibid p. 81.

its stages, so likewise will our treatment be almost diametrically opposite. We think that death not only in this, but in all fevers, rarely arises from mere debility, but from the effects of too great excitement and inflammation upon some part essential to life; our object, then, is to keep both the one and the other, if present, in a state of proper subjugation, by the steady cooperation of those remedies which are in their character strictly antiphlogistic; and we think that these, when timely and judiciously applied, will seldom fail in producing the desired effect. We do not wish to be understood as pursuing exactly the same practice in every instance; for we know that cases often occur, in which it is not necessary to bleed, or even to use very active purgation; all we wish to inculcate is, that the antiphlogistic principle of treatment is to be rigidly adhered to, whether the disease be mild or severe; leaving to the judgment of the physician the proper adaptation of the means to the *grade* of the fever. And first of

Bloodletting. The great value of this remedy in fevers has been known almost since the origin of medical science, and though amid the vicissitudes and revolutions which have been, and are still going on, it has been alternately praised as of indispensable utility, and rejected as pernicious in the extreme, its efficacy appears now to be indisputably established. Some of the ancient physicians were much in the habit of using it. Galen and Celsus thought it one of the most valuable remedies in pestilential disorders, and we are told by Prosper Albinus that the Egyptians let blood in fevers of a putrid tendency. The Arabian physicians, also, frequently made use of phlebotomy in the fevers of that country. The great advocate for bleeding in more modern times is the great Sydenham; he employed it freely in many diseases, and used it in the very fever now under consideration. After contending for the inflammatory nature of the disease and the necessity of curing it by "a suitable method and medicines," he says, that "in order to do this, I direct ten ounces of blood to be taken away from the arm, and, in effect, though the blood in this fever generally resembles pleuritic blood, yet it does not well bear repeated bleeding. But if a difficulty of breathing or a violent pain in the head in coughing, and other symptoms of this kind, show the tendency of this disease to bastard peripneumony, bleeding and purging are to be repeated, till the symptoms entirely disappear."* From this extract we learn, that in mild

* Wallis's Sydenham, vol. 2, p. 319.

cases, "repeated bleeding" was not borne well; but that if symptoms of local inflammation supervened, then the patient was to be bled and purged, till the symptoms were removed. These remarks are strikingly applicable to the fever as it appears at the present day: there are many cases which either do not require bleeding at all, or demand but the loss of a small quantity; such cases, of course, would not "well bear repeated bleeding," but, on the other hand, cases occur, such as related by Sydenham, in which we must bleed again and again, until the symptoms are subdued.

Bleeding, in severe cases of typhus, must be used early in order to derive full benefit from the remedy. If we discover from the symptoms that an acute inflammation of some vital viscus threatens the life of the patient, we cannot commence our operations too soon. We should draw blood *pleno rivo*, and not rest satisfied with a small dribbling stream; the first mode of bleeding is infinitely preferable to the last, as it effects a more speedy reduction of the pulse, and sooner brings on a state of fainting, which is much to be desired in very violent cases, as it is accompanied with vast relief to the inflamed organ.* If the heart should react under this loss of blood, and the symptoms of inflammation reappear, we should, without hesitation, open the vein a second time, and bleed until the patient is relieved. It rarely happens that bleeding thus early and decisively does not entirely remove the inflammation; and though the febrile action may continue for some days after, it is easily kept in check by the use of purgatives, &c. This vigorous practice is only intended for the most exalted grades of this fever. It is every way preferable to the half-way temporizing measures of timid physicians, who will sooner bleed ten ounces every day for a fortnight, than, by one or two copious bleedings in the commencement, extinguish the inflammation and sometimes the fever at once; and in this way producing much less debility, than by pursuing the opposite and much more precarious course. We are by no means to be deterred from this treatment by the appearances of great debility which often present themselves in the commencement of this fever; these are deceptive; beneath them lurks a disease of the most speedily fatal character; which, if not unmasked and detect-

* We have a striking example of the powerful effect of bleeding *ad deliquium*, or until a sensation of faintness or sickness is produced, in cases of violent ophthalmia. The tunica conjunctiva, which before was as red as the presence of arterial blood could make it, will be found almost completely blanched, and the pain and tension of the eye surprizingly relieved.

ed, will soon destroy the patient. The language of Armstrong on this subject is full of wisdom. "If the topical affection (says he) has been but of short continuance, and the vigor of the constitution be merely weighed down, and not really exhausted, let him discard the fears associated with false doctrines, and promptly abstract blood according to the seat and extent of the inflammation, and till local pain and general oppression be relieved."

In severe and violent cases of typhus, there is scarcely any remission, but in the milder, there is generally some little abatement of the symptoms; in such cases it is always best to use the lancet during the period of the greatest exacerbation, or acme of the fever.*

We have before said that cases of fever often occur, which do not require the lancet at all; but that they might safely be trusted to remedies whose operation, though strictly antiphlogistic, were yet inferior in point of power to venesection, such as cathartics, abstemious regimen, &c. There are likewise many cases in which the symptoms of inflammation are so slight, that though bleeding would be proper to lower arterial excitement and thus prevent an increase of the topical affection, yet a much smaller quantity will suffice for this purpose than in the more violent instances.

We trust that in the remarks made on bleeding, we have not evinced an overweening confidence in this remedy. It is certainly one of Herculean powers, and as such ought to be employed with caution. We believe the reputation of this noble remedy has often suffered from its having been mistimed, and from its having been used in insufficient quantity. It is mistimed when it is used after inflammation has continued for several days, and when irreparable injury has been inflicted on some important part; under such circumstances, we need not say that bleeding will be unavailing; nay, worse, it will only hasten death. It is, also, often used in insufficient quantity. We are too frequently palsied by apprehensions of debilitating the patient too much; "But if our fears be so great, and we take away so small a quantity of blood, how is it possible to judge exactly what good or mischief bleeding may do? For, if a disease (which requires

* It is not a little singular that some of the ancient physicians should have recommended a different course; they thought that to open a vein in the height of the fever was death; thus Celsus says, "*si vehemens febris urget, in ipso impetu ejus sanguinem mittere hominem jugulare est; expectanda ergo intermissis.*"

four pounds of blood to be taken away in order to its cure, and yet but one is taken away) destroys the patient, it does not, therefore, prove destructive because bleeding was used, but because it was performed in an improper and, perhaps, unreasonable manner.”* We have quoted the remarks of Botallus on bleeding in plague, because, we think, they are equally applicable to typhus and other fevers, and because they illustrate in a forcible manner the inadequacy of small bleedings to cure diseases which require copious abstractions of this fluid for their removal.

There are some symptoms of typhus which have been supposed to indicate great debility, and a putrid tendency of the fluids. These are petechiæ and a dissolved state of the blood. These symptoms have been supposed to forbid any thing like depletion, and to demand an opposite course of treatment. We do not deny that where these symptoms make their appearance late in the fever, when the constitution has been shattered by the unrestrained continuance of a mortal disease, depletory measures would be improper; but we by no means agree that they evince any thing like putridity of the fluids or even a tendency to it; this we consider to be totally incompatible with the laws of vitality. When, however, these symptoms show themselves early in the disease, we believe that they indicate a fever of unusual violence, and one which demands active and energetic measures for its relief. Sydenham thought that petechiæ were indicative of a highly inflammatory state of the system; for, when speaking of the benefits of a low diet and cooling regimen in this fever, he remarks, that “it must be indispensably enjoined, when the patient is attacked with a phrenzy, *petechiæ*, purple spots, or any other sign of violent inflammation, occasioned by an overheating regimen.”† Petechiæ are very often brought out by such means as excite and inflame the system. Sydenham, to whom we always refer with pleasure, remarks in his “Essay on the rise of a new Fever,” “that petechiæ frequently appear, occasioned by an unreasonable use of cordials, and a hot regimen.”‡ De Haen, also, says, that the German physicians of his day, converted common fevers, by the use of the “regimen calidissimum,” “into the petechial and miliary.”|| On the contrary, symptoms indi-

* Botallus on Plague. Wallis's Syd. vol. 1, p. 130.

† Wallis's Syd. vol. 2, p. 325.

‡ Ibid, p. 317.

|| Thes. Sist, p. 35.

cative of a malignant putrid character, are moderated or prevented by pursuing an antiphlogistic and cooling plan of treatment. In proof of this we have evidence handed down to us, by the bright luminaries of the profession in times that are gone. Sydenham, after contrasting the two methods of treatment, says, "so, likewise, if I find, in the fever under consideration, that the more the patient is heated, the more he is disposed not only to phrenzy, purple spots, *petechiæ*, and the like symptoms; but farther, that the fever, by this procedure, is attended with all sorts of irregular and violent symptoms; and, on the other hand, if it appears that another patient, by treating him according to the method here proposed, (i. e. the antiphlogistic plan,) is quite free from such symptoms, reason shows that the latter method of practice is much the best."* De Haen, who was a strong advocate for the inflammatory nature of this fever and, of course, for venesection, says that *petechiæ* never appeared in it when he had the management of it from the beginning. "Liceat ne addere, (says he,) quod et medici complures et ego in nosocomio, sive in vigore morborum, sive eorundem in fine, nunquam nostris in ægris, quibus a principio affueramus arbitri, miliaria deterimus;"† and in the same work expresses his belief, that *petechiæ* would but seldom appear, even in the plague itself, if the antiphlogistic method of Botallus and Sydenham was strictly followed. Dr. Dickson, in his remarks on the contagious fever which prevailed on board the Russian fleet, after mentioning that the appearances on dissection of those who died of this fever were "strongly illustrative of the frequency of visceral inflammation," observes "that from reflection on numerous facts, it seemed to him an

* Wal. Syd. vol. 2. p. 354-5.

† Although, in the above extract, the military eruption is mentioned as being prevented from appearing by bleeding, &c., yet as De Haen considered this eruption as nearly the same with the *petechiæ*, and that those means which prevented the one would also prevent the other from appearing, and that the same remedies would equally remove both when present, we have taken the liberty to quote it in this place as equally showing the effect of bleeding in preventing the appearance of *petechiæ*. That De Haen considered the petechial and the military fever as very nearly related, and that the remarks he made upon one were equally suited to the other, is perfectly obvious from the following extract taken from the end of his section de Febre Petechiali. He says, "Multa de petechiis dicenda supersunt; maxime de iisdem tum proveniendis, antequam fiant; tum cum adsint, curandis; verum cum hoc quoque ad miliarium eruptionem pertineant, ipsaque miliarium historia eam pectarum elucidet, atque explanet, una fidelia hunc utrumque parietem dealbabo." This quotation, we hope, will be a sufficient justification for using the above extract as we have done.

inevitable conclusion, that those remedies which arrest inflammation at the commencement, prevent the graver and malignant symptoms, which characterize the last stage of such fevers; and this conclusion was amply borne out by the results of his practice, since bleeding and purging at the beginning were generally successful."* Rush, in his "Defence of Bloodletting," remarks that Dr. Morton describes what he called a putrid fever, which was epidemic and fatal, in the year 1678. Dr. Sydenham, who practised in London at the time, takes no notice of this fever. "The reason of his silence (says Rush) is obvious. By copious bleeding, he prevented the fever of that year from running on to the gangrenous state, while Dr. Morton, by neglecting to bleed, created the supposed putrid fevers which he has described." The importance of this subject will, we hope, be a sufficient apology for our having dwelt on it at some length. We consider it of great importance in a practical point of view to have correct notions on this subject. If we adopt the opinions of those who consider the appearance of petechiæ as at all times denoting debility, and a tendency to putrefaction in the fluids, and on this account to be treated by tonics and stimulants, we will most unquestionably do much injury. From our own experience, and from the testimony of others, we firmly believe that, when occurring early in the fever, they indicate a high grade of disease, which must be combated by active measures of treatment,† and so long as the pulse preserves its strength, we may deplete regardless of their presence.

As to the dissolved state of the blood, if it occurs early in the disease, and the other symptoms do not forbid the use of the lancet, we should proceed as though it did not exist. Rush considered it as indicating a highly inflammatory state of the system. In his history of the yellow fever of 1793, he says, "I paid no regard to the dissolved state of the blood, when it appeared on the first or second day of the disease, but repeated the bleedings afterwards in every case, when the pulse continued to indicate it. It was common (says he) to see sizy blood succeed that which was dissolved."

* Armstrong on Typhus, p. 154.

† Doctor Rush, in his account of the yellow fever of 1793, remarks, that "the presence of petechiæ did not deter me from repeated bloodletting, when the pulse retained its fulness or tension." After mentioning instances of its success, he says, "I find precedents in De Haen and Dr. Sydenham in favor of the practice. So far from viewing these eruptions as signs of putrefaction, I considered them as marks of the highest possible inflammatory diathesis. They disappeared in each of the above cases after bleeding.

Cathartics. Cathartics, next to venesection, are, unquestionably, the most important remedies in this fever. Unlike bleeding, however, they are not only admissible, but, in a greater or less degree, are demanded, in every grade and in almost every stage of the disease. The ancient physicians were much in the habit of employing purgatives in fever and other diseases; the number of such articles then in use was comparatively limited, and they were very rough in their operation. Sydenham, in more modern times, carried the practice of purging to a very considerable extent. After his time, however, and previous to the revival of the practice of purging in typhus by Hamilton, cathartics had been in a great measure neglected in the cure of fever; particularly in that under consideration. Brown, in accordance with his belief of the asthenic character of typhus, of course, rejected them; and Cullen placed but little confidence in them, and generally substituted enemata in their stead.

We have said above that Hamilton revived the practice of purging in typhus. This assertion has not been made without due consideration, and only from a conviction that we ought, in matters of science and discovery, as in other things, to "render unto Cæsar the things that are Cæsar's." Hamilton certainly deserves great credit for shaking himself loose from the doctrines of his day; but he has no claims to originality in prescribing purgatives in typhus fever. If we refer to Sydenham, we shall find that he used them freely; and in the very disease now under consideration placed his chief reliance on bleeding and purging for its cure. In his "Essay on the rise of a new Fever," (which is, as was before mentioned, the typhus of the present day,) after recommending the use of purgatives and bleeding, he remarks that, "the method just recommended is the best I ever tried in curing this fever; and if it fails of effectually removing it, at least brings it to intermit, and then it always yields to the bark. But as purging, as it is here directed in order to cure this fever, may perhaps seem detrimental to some persons, I assert from experience, that nothing cools so much and so surely as purging after bleeding." After assigning some reasons for their beneficial effects, he says, "For these reasons, therefore, I hope I may assert, on good grounds, that *the method of cure above delivered, which consists in bleeding and purging, is the most effectual one to conquer most kinds of fevers;*"* and in other passages he enforces the same practice. It will be seen from the above extracts, that

* Wallis's Sydenham, vol. 2, p. 344-5.

nearly a century anterior to the time of Hamilton, Sydenham prescribed purgatives in this disease from the beginning, and recommended them, conjoined with venesection, as the best cure for most kinds of fever.*

Hamilton, it seems, was led to the employment of purgatives by mere accident: he says, "At the time I was appointed physician to the Royal Infirmary, the cure of typhus was thought to consist chiefly in the removal of atony and spasm of the vessels of the surface of the body. For this purpose, among other remedies weak antimonial wine and nauseating medicines were freely given. The state of the stomach and bowels, after the exhibition of an emetic and purgative on the first approach of the attack, was little regarded in the after periods of the fever. An occasional stool was procured by a mild glyster, while a purgative medicine was given with extreme caution. Apprehensions were entertained that the operation of a purgative would rivet the spasm of the extreme vessels, and increase debility, one of the supposed direct causes of death in fever. These apprehensions (continues he) may still bias the practice of many, as they certainly did bias mine, for a long time." An unusually malignant typhus fever, however, breaking out in Edinburgh, in the summer of 1779, and being unable to cure it by the mild antimonials, Hamilton was induced to resort to the *calx antimonii nitrata*. "This antimonial remedy (says he) was not ineffectual; but I remarked that it was beneficial only when it moved the belly. The stools were black and fœtid, and in general copious. On the discharge of these the low delirium, tremors, floccitatio, and subsultus tendinum, which had prevailed, abated in some cases; the tongue, which had been dry and furred, became moister and cleaner, and a feeble creeping pulse acquired a firmer beat. Reflecting afterwards on these cir-

* It has, we think, been too much the custom to overlook the practice of Sydenham, and to consider his great excellence to consist in his unrivalled descriptions of acute diseases. We, however, look upon his writings in a very different light: he certainly deserves great credit for his accurate account of diseases, but we by no means consider it as his highest praise. The improvement he made in the treatment of smallpox is enough to immortalize any one man, and his mode of curing fevers and the phlegmasiæ has been but little improved on even at the present day. There is one striking instance in which his claims to originality of treatment are almost entirely overlooked, and all the credit assigned to Hamilton. I allude to the method of curing St. Vitus Dance. He used identically the same routine of practice in this disease which is followed at the present day upon the authority of Hamilton. Any one may satisfy himself of the truth of this by consulting his works.

cumstances, it occurred to me, as the purgative effect appeared to have been the useful one, that any purgative medicine might be substituted for the calx antimonii nitrata, and that by this substitution, the unnecessary debilitation of an exhausted patient, by vomiting and sweating, might be avoided."

"More extended experience confirmed these conjectures; and I was gradually encouraged to employ purgative medicines early in typhus, and to repeat them in the course of the disease; and after having long and strictly directed my attention to this point of practice, I am now thoroughly persuaded that the full and regular evacuation of the bowels relieves the oppression of the stomach, and mitigates the other symptoms of fever."*

These extracts are interesting and useful, as they give the experience of an accomplished and practical physician; and as showing the gradual dawning of light on a mind clouded and fettered by the scholastic doctrines of the day.

Huxham, however, anterior to the time of Hamilton, had remarked the great and surprising relief experienced from the free discharge of dark bilious matter from the bowels. In his essay on "malignant putrid fever," he remarks, that "unquestionably the bilious principle is too greatly predominant in all putrid, malignant, and petechial fevers. The gall bladder, and biliary ducts, are always found full of black or green bile in those that die of pestilential diseases, and so is the stomach, duodenum, &c. Now, (continues he,) if this putrid bile is not carried off, it grows more and more corrupt, and causes vast anxiety, sickness at stomach, pain, &c.; and, being reabsorbed into the blood, creates infinite evils, greatly irritates the *genus nervosum*, destroys the *crisis* of the blood, and turns the lymph into a corrosive ichor. When, therefore, there are signs of its being redundant, it should be forthwith discharged by vomit or stool, as nature points out. I have many times, with the greatest pleasure, in these putrid fevers, seen an amazing change for the better immediately succeed a fit of vomiting, and a stool or two, when an inexpressible anxiety, load on the *præcordia*, perpetual sickness, *eructation*, and *singultus*, had preceded. The extreme foulness of the tongue, and load at the stomach, with a loathsome bitter taste, and a horrible offensive stinking breath, and eructations, show the condition of the stomach; and the abominably fœtid, black, bilious stools, the necessity and advantage of that discharge."† This extract

* Hamilton on Purgatives, p. 29.

† Huxham on Fevers, p. 94.

from Huxham, though a long one, is, we conceive, highly valuable, inasmuch as it informs us of the state of the gall bladder, biliary ducts, stomach, and duodenum, after death from this fever, and mentions the effect which a retention and reabsorption of this dark bile has upon the genus nervosum. The first points out the indispensable necessity there is for cathartics, and the last teaches us that many of those nervous symptoms incident to the disease, and which have been supposed to proceed from debility, and treated as such, are to be attributed to a retention of this black bile, and can only be relieved by its evacuation.

Even among those, however, of the present day, who agree upon the utility of free purgation in this fever, there is much diversity of opinion as to the kind of cathartics which are best adapted for this purpose. While some proscribe the use of calomel and all other active purges, and rely on laxatives almost entirely to effect the evacuation of the bowels, there are others who look upon it as the sheet anchor of hope, not only in this, but in most other fevers, and prescribe it with a degree of boldness and daring perfectly astonishing to those who are accustomed to administer it only in five or ten grain doses. Either practice may be carried to extremes, but of the two, in severe cases of fever, we should greatly prefer the bold exhibition of active remedies to the milk and water practice of those who would altogether exclude such means from the treatment of fever. We have always been the steady and constant friend of the use of calomel in fever; but in ordinary cases we believe its moderate to be just as beneficial as its extravagant exhibition, with this advantage, that there is less risk of producing that painful, disgusting, and sometimes fatal affection of the mouth, a salivation. This Hercules of the *materia medica* is not to be rashly dealt with, without often leaving behind the most lamentable and distressing consequences of its maladministration. When judiciously used, however, it may truly be said to be an invaluable remedy; there is no article in the whole *materia medica* which so effectually discharges that dark bilious matter, which affords so much relief in fever. It is customary to combine it with some other cathartic, and we know of none better than jalap; ten grains of each for an adult, administered every two hours, until the bowels are freely evacuated, constitutes as effectual a purge as can be used. This combination was much employed by Dr. Rush in the yellow fever of 1793, and it was then called a dose for a horse; what would they who so called it have thought of sixty grains of calomel as a common dose? Jalap, however,

to some stomachs is so exceedingly nauseating and offensive, that we are obliged to substitute something in its stead; this may be done by adding the same amount of scammony to the calomel, and administering it in the same way. Scammony has hitherto been but little used on account of its supposed drastic and harsh operation, but from repeated experience we can declare that it is less so than jalap, and is equally effectual in evacuating dark bilious matter. Another mode that we have found very effectual in evacuating the bowels, and lowering the action of the heart and arteries, is to administer calomel alone, and some time afterwards give a strong infusion of senna, with either Epsom or Glauber salts. This generally operates most copiously, the discharges being very thin; it is, therefore, only suited to the beginning of the disease, when the strength is but little impaired, and to those of strong constitutions; it is very apt to prostrate the thin and delicate too much. The cathartics are to be repeated day after day, until the fever is subdued; when, however, it has continued for some time, and the patient is much weakened, we generally combine aloes with the calomel and jalap, to render the discharges more consistent. The following is the prescription in general use:

Calomel,	3ss.	} M. D. in pil: xxx.
Jalap,	3i.	
Aloes,	3i.	

Four or five of these pills given, and repeated according to circumstances, will generally operate freely enough. The discharges produced by this combination, are much more consistent, and, of course, less debilitating, than those by calomel and jalap alone. If, at any time during the exhibition of these pills, the mouth should become sore, the calomel must for a while be discontinued, and the same quantity of aloes and jalap be given alone; this combination is the very best substitute for calomel that we are acquainted with. When the patient is fairly convalescent, it is necessary to pay much attention to his bowels, if they should not be sufficiently open; then two or three of the above mentioned pills ought to be given. There are, however, other articles, which, for this latter purpose, answer very well, such as castor oil, magnesia, &c.

The above practice is suited to those cases in which the bowels are easily operated on. Cases, however, of great torpor sometimes occur, in which it is impossible to obtain an operation by the usual doses. In such, it is idle to limit ourselves to a certain number of grains; we must exhibit cathartics in doses *sufficiently large to purge*, for without this the

patient will die ; we believe that the safety of the patient in fever, depends upon a continued evacuation of bile from the bowels, and keeping the action of the heart and arteries in proper control by the other antiphlogistic means ; we must then judge of the necessity for an increase of the dose, by the effect produced, and not by the number of grains given. Fortunately cases of such extreme torpor of the bowels are rare ; but when they do occur, they are only to be managed by the most bold and intrepid exhibition of medicines ; in the language of Hippocrates, “ad extremos morbos, extrema remedia sunt exquisite optima.” The next remedy in point of importance is

Cold Water. Cold water was much used, both internally and externally in fever, by the Greek and Roman physicians, some of whom relied upon it exclusively as a means of cure. We find mentioned, by both Celsus and Galen, that Petro, a physician who flourished a considerable time after the death of Hippocrates, attempted to cure fever by copious draughts of cold water. It appears from the description of his method, given by Celsus, that in cases of fever, he first caused the patient to be covered with a good deal of clothing, in order that he might excite in him great heat and thirst, and then commenced the profuse administration of cold water.*

The use of cold bath was introduced at Rome during the reign of what was called the methodic sect. From its having been first successfully used in the case of the emperor Augustus, whose complaint had baffled all the usual applications, it acquired great reputation, and was subsequently used with considerable boldness by some of the Greek and Roman physicians. The Arabian physicians, also, employed it much, both internally and externally, in pestilential diseases. The practice, however, was, in progress of time, lost. During the last century its use was revived by doctors Jackson and Wright.† The following is Dr. Jackson’s account of the

* Liquidem apud antiquos quosque ante Herophilum at Erasistratum, maxime post Hippocratem fuit Petro quidem, qui febricitantem hominem ubi acceperat, multis vestimentis operiebat, ut simul calorem ingentem sistimque excitant. Deinde ubi paulum remitti ceperat febris, aquam frigidum potui dabat.” Celsus Lib. iii. e. g.

† We find it asserted in vol. i. p. 89 of Johnson’s valuable work on Tropical Climates, that “it has been practised in the Bengal remittent, time immemorially, among the natives themselves, many a century before a Jackson, a Wright, or a Currie thought or wrote on the subject.” To prove this, he introduces an extract from the Oriental Field Sports, by captain Williamson, which we will copy here. “We must, however, (says captain Williamson,) do the natives the justice to allow, that the refrigera-

circumstances which led him to the employment of cold bathing. He says, that "the first hints of this practice were accidental, and arose from a conversation I had with the master of the vessel in which I went passenger to the West Indies. This person commanded a transport in the war of 1756, and was present at the siege of Havana. As he was talking one day of the state of the fleet, he mentioned accidentally, that some men were sent aboard of his ship ill of fevers; several of whom, he observed, jumped into the sea during the delirium which attended the paroxysms of the disease. Some of them, as might be expected, were drowned; but the most part of those who were recovered from the waves appeared to be greatly benefited by the ducking. The fact, which, from the veracity of the man, I thought I could depend upon, struck me strongly, and I resolved in my own mind, to bring it to the test of experiment as soon as an opportunity should offer. Neither was it long after my arrival in Jamaica, that I had occasion to visit a sailor whose situation seemed to justify such a trial. The poor man was aboard of a ship, which lay at anchor about a mile from the shore. He had been ill two days; the delirium ran high; his eyes were red and inflamed; his respiration was hurried; he was anxious and restless in a high degree, while, together with those marks of excitement, he was occasionally languid and disposed to faint. His skin being dirty, furnished an ostensible excuse for trying this remedy. But it was previously thought proper to draw some blood from the arm; which being done, some buckets of salt water were dashed on the shoulders. He was now laid in bed; a copious sweat ensued, succeeded by a distinct remission and a total change in the nature of the symptoms." After relating some other instances of its astonishing efficacy, he says, "I shall only add, that I have tried the remedy, in various situations, always with safety, generally with astonishing success; so that I cannot forbear recommending it even at an early period, in

ting principle lately adopted by some of our leading physicians, owes its origin solely to the *ancient practice* of the Brahmins, or Hindoo priests, of whom the generality affect to be deeply skilled in pharmacy." And he then adds, "I believe that, if taken in time, few fevers would be found to degenerate into typhus, and that very seldom any determination towards the liver, in acute cases, would occur, were the refrigerating course to be adopted. Often (says he) have I known my servants, when attacked with fever, to *drink cold water* in abundance, and to *apply wetted cloths to their head*, with great success. The former has generally lowered the pulse considerably by throwing out a strong perspiration, while the latter has given immediate local relief."

the fevers of the West Indies.”* In the year 1777, Dr. William Wright, of Jamaica, published an account of the salutary effect of ablution with cold water upon himself and others, while laboring under severe fever. This narrative of Dr. Wright excited much interest and drew the attention of many distinguished men to the subject. Among others, Dr. Jas. Currie, of Liverpool, entered into it with much zeal, and soon after enjoyed extensive opportunities of testing its efficacy. On the ninth of December a contagious fever made its appearance in the Liverpool Infirmary, and spread itself rapidly. In this fever he, for the first time, commenced the use of cold ablution, and out of seven cases treated by it not one died; encouraged by his success in this first trial of the efficacy of cold water, he continued to use it freely, both in hospital and private practice, with extraordinary success; and some time after published his admirable work on the use of cold water, in which are contained the results of his extensive experience with it, and the rules which should guide us in its use.

No one can peruse this volume without being convinced, that cold water, when properly applied, is a most important remedy in fever. Its utility is not confined to typhus; it is equally serviceable in all fevers attended with increase of heat and arterial action. Its effect upon the pulse is astonishing in many cases. We have often known the mere bathing the arms and hands of a febrile patient reduce the action of the pulse ten or fifteen beats in the minute; and if this partial application of cold water has such an effect on the action of the heart, how much greater must be the effect of a cold bath? We have many instances on record of its calming at once the most furious delirium; persons in such a situation have often jumped overboard a vessel into the sea, and been taken up perfectly calm and rational, and with an almost complete extinguishment of the fever. With the many strong instances recorded in various works, of its remarkable efficacy in curing fever, it is justly a matter of surprize, that physicians so seldom call its great powers into requisition. It exercises a more immediate control over the action of the heart than bloodletting. Dr. Currie mentions a striking instance of the effect of cool air in reducing the pulse. “In the month of May, 1801, (says he,) I was desired to visit a patient ill of fever in Sparling street. I found him in the tenth or eleventh day of the disease, delirious and restless; the surface of the body dry, and his heat 104° of Fahrenheit. The room was close, and I desired the only window in it to

* Jackson on Fevers.

be opened. The wind from the northwest blew directly into this window, and the bed being situated between it and the chimney, a pretty brisk stream of air passed over it. The patient had just thrown off a considerable part of his bed clothes, and was exposed naked to the breeze. I sat by him, with my finger on his pulse, watching the effect. In a little time the pulse fell from 120 to 114 in the minute; he became more tranquil, and soon afterwards he sank into a quiet sleep, in which he remained when the water for affusion was prepared; of course, we did not disturb him;* he remained exposed to this cold air until morning, when his pulse was found to be about 100, and his heat 101.

Dr. Rush, in his history of the yellow fever of 1793, relates a similar case to the above. "Dr. Griffiths (says he) furnished a remarkable instance of the influence of cool air on the fever. Upon my visiting him, on the morning of the eighth of October, I found his pulse so full and tense as to indicate bleeding; but after sitting a few minutes by his bed side, I perceived that the windows of his room had been shut in the night by his nurse on account of the coldness of the night air. I desired that they might be opened. In ten minutes afterwards the doctor's pulse became so much slower and weaker that I advised the postponement of the bleeding, and recommended a purge instead of it;" and in the next page he remarks, that "cold water, when applied to the feet, as certainly reduces the pulse in force and frequency, as warm water applied in the same way produces contrary effects upon it. In an experiment which was made at my request, by one of my pupils, by placing his feet in cold pump water for a few minutes, the pulse was reduced twenty-four strokes in a minute, and became so small as hardly to be perceptible." These facts teach us, that it is the cold alone which is serviceable, since it has the same effect whether applied in the shape of water or air; and they also teach us the powerful influence it has over the action of the heart, and the great value of its proper use in the cure of fever.

Cold water may be used either internally or externally, or in both ways. It has been already mentioned that Petro relied entirely on copious drenching with cold water for the cure of fever; and about the middle of the last century (1752) a treatise was published on the internal use of cold water, under the title of "*Febrifugum Magnum*," by Dr. Hancock London. The most effectual way, however, to derive full benefit from the action of cold, is to use it in both

* Medical Reports, p. 259.

ways ; the same principle which would induce us to resort to affusion, would also suggest its internal administration. The latter is generally so consonant with the patient's inclinations, that there is usually no difficulty in its application, and, therefore, it should always be recommended ; the affusion, however, is looked upon by society as disagreeable if not hazardous, and hence, in proposing such a remedy, we have generally to encounter much prejudice. In severe cases, peremptorily requiring the use of active means, we should disregard this feeling ; but if otherwise, we may be satisfied with its partial external application. We can always have the feet, arms, and hands bathed in cold water, and cloths dipped in the same applied to the stomach and head. This affords very great relief, and ought never to be omitted.

But cold water is by no means to be used indiscriminately in every case of fever ; neither is it to be used in all stages of any fever ; the rules which Currie has laid down on this subject are excellent, and cannot be followed too closely. If we obey strictly his directions, we will always be prevented from misapplying or doing injury by its use. He gives separate rules for the external and internal use of cold water ; but as its effect, except in degree, is the same when used either way, so one set of rules will answer as a guide for both. His first general rule is, that “ It may be used (either internally or externally) *when there is no sense of chillness present, when the heat of the surface is steadily above what is natural, and when there is no general or profuse perspiration.*” We will now give the substance of the particular rules he has laid down on this subject. 1st. “ Cold water is not to be used either internally or externally in the cold stage of the paroxysm of fever, however urgent the thirst ;” taken at such times it increases the chillness and produces great weakness of the pulse, and if used to any extent might cause the death of the patient. 2d. When the hot stage is fairly formed, and the surface is dry and burning, cold water may be used both ways with the utmost freedom ; frequent draughts of cold liquid, and its external application, under such circumstances, are highly grateful ; they diminish very much the heat of the body, and lessen considerably the volume and frequency of the pulse. 3d. “ It is also necessary to abstain from the use of cold water when the body is under profuse perspiration, and this caution is more important in proportion to the continuance of this perspiration.”

We will finish what we have to say on the use of cold water, by a few remarks on its *modus operandi* in curing fever. And in the very commencement we cannot help expressing

our surprize, that cold should ever have been considered as a stimulant. If it be such, then is Currie's general rule for the use of cold water an absurdity; for a stimulant would do no injury during the presence of a chill, and would be obviously prejudicial in the heat and exacerbation of fever. However wild and imaginative Brown may have been in many of his notions, his opinion that cold can never act as a stimulant is unquestionably correct. It is so plain a matter that it is unnecessary to argue much about it. We will only state a few positions, the truth of which none can deny. In the first place, if we take a person in health and immerse him in cold water, and then feel his pulse, we will invariably find it small and weak, and it will continue so as long as he remains in the water. 2d. If we immerse a person laboring under a chill, when the pulse is already weak, it will be rendered much weaker and has sometimes been extinguished. 3d. Currie relates many cases of fever, in which the application of cold greatly reduced the action of the heart; in one case, in a very short time, the pulse was diminished eight beats in the minute; and in the case mentioned by Rush, the pulse was lessened twenty-four strokes in a minute, by the immersion of the feet in pump water. These facts unquestionably prove that cold has none of the properties of a stimulant, since it weakens the pulse in a state of health, weakens it still more when already weak, as in a chill, and lessens very much its volume and frequency when in a state of excitement, as in fever. "But," say the advocates for the stimulant property of cold, "if you take a person out of the cold bath, his pulse rises, his skin becomes warm, and his cheeks flushed, and hence we argue that it stimulates the system." In answer to this, we remark, that one might just as well contend that brandy was sedative in its character, because a man that had been intoxicated by it was rendered sober by its withdrawal, as to argue that cold is a stimulant, because the heat reacted when removed from the sphere of its influence. So long as we keep a person in the cold bath, so long will his pulse continue small and weak; but if we remove him from the bath, then his pulse rises; and from what cause? Surely not from the cold, because, having been withdrawn, it has, of course, ceased to operate; but from the increased accumulation of blood about the right side of the heart, which is the necessary and inevitable result of its weakened action, which last is produced by the cold application. We have then a sufficient cause for this increased action of the heart and flushing of the cheeks, in the presence of an unusual quantity of the heart's natural stimulus, without attributing it

to cold, which, as we said above, has a directly contrary effect, so long as its application is kept up. These are some of our reasons for considering cold not to be a stimulant. The same facts or reasons, however, which prove it not to be stimulating in its properties, prove it to be sedative in its character. It is from its operating in this way, that we can readily account for its extraordinary effect in fever. It is by diminishing suddenly and very greatly the action of the heart, that a cold bath will calm, almost in an instant, the most raging and unmanageable delirium.

Blisters. Much variety of opinion exists as to the utility of blisters in fever. While some recommend them early in the disease,* others confine their application to its advanced stage, and some there are who reject them in toto;† and there are others still, who think them only beneficial when applied to relieve some topical affection. We are among the latter number. In cases of fever attended with inflammation of some important part, we have often witnessed the most remarkably good effects from their use, and never fail to apply them under such circumstances. We entirely agree with those who deprecate their early application in continued fever, not characterized by some local affection, and even in such cases we should always prefer using free depletion before resorting to them. When applied without the presence of inflammation, they are decidedly injurious; they increase the fever and irritation of the patient, and often prove a source of much torment to him. The language of Fordyce on this subject is exceedingly judicious, and ought to have great weight with all those who believe that experience is the best teacher, particularly in matters relating to medicine. Fordyce was probably as conversant with fever as any man of his day, and therefore when he speaks we should listen with attention. He says, that "Whether exciting inflammation has or has not the same effect in a regular continued fever, which it has in health, can only be known by making the application to the body of a person affected with regular continued fever. As far as the author's experience goes, when

* Lind remarks, that "In a moderate infectious fever, where the source of infection is not very violent, if twenty patients be blistered, sixteen will next morning be entirely free from headache, heat, pain, and fever."

† Dr. Moore, in his "Medical Sketches," observes, that "Notwithstanding my having watched the effect of blisters with all the attention I am capable of, and formerly with a strong prepossession in their favor, I cannot assert that I ever knew vesications of any use in this disease, (i. e. typhus;) but I have frequently seen the patient teased by their irritating quality, without their seeming to have any other effect."

any stimulus has been employed so as to produce inflammation, when the patient has become weak towards the end of a continued fever, the only difference which has occurred has been, that phlegmonous inflammation has not produced hardness, fullness, and strength of the pulse, but both phlegmonous inflammation and inflammation of the skin have occasioned greater frequency of the pulse, have rendered it weaker and smaller, and as in health have prevented sleep, and the patient's taking the same quantity of nourishment, and have depressed and deranged the whole system."* Huxham, an old author; and on this account many may think his opinions not entitled to much weight, though we are free to confess that we think not the less of him for that reason, has some excellent remarks on this subject. "What I have said (observes he) of volatile alcalious salts, leads to a reflection on the promiscuous use of blisters in these fevers, which by some are deemed the only *anchor of hope* in such dangerous cases; but I think they are many times too hastily and improperly applied, especially in the beginning, when the fever runs high, and doth not demand a farther *stimulant*; for the action of cantharides is not confined to the skin, but affects the whole nervous and vascular system: now when the irritations and vibrations are already too great, as frequently happens in the beginning of such fevers, they are very injudiciously applied. It is true, indeed, (continues he,) nature may sometimes want a *spur*, nay, often doth so, particularly towards the decline of these fevers, when the solids grow torpid, the circulation languid, the spirits affete, and the sick comatose; but in the above circumstances I have very many times seen very pernicious effects attend their too early application, as obstinate *pervigilium*, *delirium*, suppression of urine, *tremors*, *subsultus*, &c."† We find from this extract, that, though Huxham disapproves of the use of blisters in the early stage of fever, yet he acknowledges the occasional necessity for their use in the last; yet even under the circumstances he has mentioned, we should greatly prefer the use of sinapisms, because they are much more powerful and more immediate in their effect, and are not so apt to produce a troublesome and tormenting sore; this last effect we have often known to occasion much fretfulness and pain to the patient during the whole stage of convalescence. Another objection to the use of blisters in the last stage of this fever, when the debility is extreme, is the probability of their producing gangrene in the part to which they are applied. Sir John Pringle, Clark,

* Fordyce on Fever, p. 270.

† Essay on Malignant Putrid Fevers.

and Lind, and many others, bear testimony to this effect of blisters, when applied to patients under such circumstances.

From the opinions of the distinguished physicians which we have quoted, and from our own experience, we are justified in the conclusion that blisters are inapplicable in the early stage of fever, and that they are rather of equivocal benefit when applied in its advanced stages; used, however, at any stage to relieve topical inflammation, they are of eminent service; they always do most good, however, when resorted to after the pulse has been reduced by proper depletion, and this, indeed, ought always to be done.

When blisters are used to relieve local inflammation, they should be applied immediately over the part affected, or if this be impracticable, then as near to it as possible. Blisters frequently produce a very unpleasant affection of the bladder, called strangury; this can be most generally relieved by drinking freely of flax seed tea, barley water, &c.; if this should fail, we can recommend, as an infallible remedy, the injection of five, ten, or twenty grains of camphor, suspended in about a gill of mucilage of gum arabic.

Bark and Wine. These two remedies were formerly much relied on for the cure of typhus fever, and even at the present day there are many who adhere to this mode of treating the disease. It is, however, one of the relics of exploded error which will soon cease to exist, and is only to be found in those who are fettered by educational prejudices, which they are unwilling or unable to break. If this disease be sthenic in its character, then it necessarily follows, that tonics and stimulants are not the proper remedies for its cure. We believe that bark is never necessary in this fever, unless when it changes to an intermittent type, and here it may be given with excellent effect. We suspect strongly that when bark cures this or any other disease of excitement, it does so by acting as a purge; for we know that when given in any quantity it very frequently acts in that way. Bruce says that bark cured the bilious fevers of Massuah when it operated as a purge. Stimulants we reject not in toto, but almost entirely in the cure of fever we feel the most unrelenting prejudice against them; for it has fallen to our lot to see many cases in which irreparable injury was produced by their improper and unreasonable exhibition.* We have frequently known patients stimulated into a delirium,

* Stimulants are injurious in proportion to the violence of the fever, and hence they are so destructive in the high grades of the autumnal fevers of this country. Rush deprecated, with all his earnestness and enthusiasm.

and sometimes into apoplexy; and we have known them rescued from the latter condition by the supervention of a free bilious diarrhœa, a most convincing proof that the supposed debility of the patient, to counteract which stimulants were resorted to, did not demand such treatment. It is that bugbear debility which frightens us into too early a resort to remedies of such pernicious tendency; and until we can divest ourselves of the fears arising from this delusive source, we will often do wrong by administering them at an improper period of the disease.

However strongly we may be opposed to stimulants as constituting a necessary part of the general treatment of fevers, we yet think them highly important in their proper place, and of indispensable utility under certain circumstances. When a patient is very much prostrated from the long continuance of fever, we may often unite, with great advantage, some stimulant to the means used for subduing the fever. Camphor and wine are the two most frequently resorted to. We greatly prefer the latter to any other kind of stimulant; the camphor, when used in any quantity, almost always producing a most unpleasant delirium. Of the different wines, Madeira is much the best. Armstrong says, that in the advanced stages of typhus he is in the habit of mixing milk with the wine, and under this form he thinks it makes an excellent drink.

It will be seen by the above remarks, that we are decidedly opposed to stimulants, in every stage of the fever, except under the circumstances mentioned. The majority of cases of typhus, when treated properly in the primary stages, will not often require the use of stimulants in the last. Hamilton, some years after he commenced the purgative plan of treatment, says, "I cannot, however, omit remarking, that for some years past I find wine less necessary in fevers than I formerly thought it was;" and every unprejudiced physician, who properly employs depletory measures in the beginning of fever, will acknowledge that stimulants are but rarely necessary in the last stage.

Of the Diet and Drinks. We have chosen to consider the subject of regimen under a separate head, because we think that a proper regulation of it has considerable influence

their exhibition in yellow fever, and some of the older authors also mention their fatal effects in fevers. Thus Shenkins, in speaking of the famous Hungarian fever, which prevailed so extensively in Europe about the middle of the sixteenth century, says, "omnes qui vini potione non abstinuerunt, interiere, adeo ut summa spes salvationis in viui abstinentia collocata videtur."

over the state and progress of the disease. From much attention to this subject we are convinced that fever is often kept up and protracted by improper articles of diet; the friends of the patient usually betray such excessive anxiety upon this subject, are so much afraid of his dying from mere inanition, and are generally so importunate in their intracacies for more nourishment, and so much dissatisfied with a refusal, that few physicians have fortitude enough to enforce the dietetic plan as rigidly as they could wish, and as would best comport with the interests of their patients. The rigid enforcement of a very low diet in fever is, we think, the only respect in which Broussais' practice ought to be followed.

The ancient physicians were well aware of the great importance of a suitable diet in fever, and accordingly paid much attention to it. Hippocrates permitted the patient to drink freely of cooling beverages, and gave but little nourishment during the first days of the disease. Erasistratus, who lived about two hundred years after Hippocrates, being of opinion that fever proceeded from too great plethora, enjoined a general and rigid abstinence upon his patients. Asclepiades and his followers also prescribed abstinence, but limited its duration to three days; he permitted the patient neither to eat nor drink during this time, and says that in this way the fever was cured *tuto celeriter et jucunde*. The practice of limiting the abstinence strictly to three days was distinguished by the name of diatrition, and those who followed it by the appellation of diatritarii. We find, therefore, from what has been mentioned, that the ancient physicians thought a proper diet a matter of considerable importance in fever.

The regimen during the fever should be of the lightest and most abstemious character. We do not think that we can well err by the utmost strictness on this point; we should literally starve the fever. Animal food should be proscribed as pernicious in the extreme. A cup of barley water, or of weak tea or coffee, twice a day, with a hard water cracker, if the patient should desire it, is generally sufficient for him; he should also be allowed to drink freely of cooling beverages, such as lemonade, apple water, rice water, currant jelly water, toast and water, balm tea, and cold water; here is variety enough to satisfy the most wayward and capricious appetite, and which can be indulged in freely without the fear of injury.

But it is not during the febrile state alone, that we should be particular about the diet of the patient; much attention to it is also necessary during the stage of convalescence. The patient should be permitted to return gradually to his

former mode of living, commencing with the lightest and most digestible articles of diet, such as arrow root, sago, tapioca, &c. Animal food ought not to be allowed until he is able to walk about; a too early use of the latter is often the cause of severe relapses, and we are earnestly cautioned against it by most writers on fevers. Thus Huxham, in his account of the malignant fever which prevailed at Plymouth in the year 1740, remarks, that "If any one made use of a flesh or a fish diet, before he had been very well purged, and his recovery confirmed, he infallibly indulged himself herein at the utmost danger of his life;" and Dr. Poissonnier, in his work on the maladies of seamen, says, that "the physicians of Brest have observed, that the relapses in the malignant fevers which prevailed in their naval hospitals, were as much the effect of a fault in the diet of the sick, as of the contagious air to which they were exposed, and that as many patients perished from this cause as from the original fever. For this reason light soups, with leguminous vegetables in them, panada, rice, seasoned with cinnamon, fresh eggs, &c., are all they should be permitted to eat. The use of flesh should be forbidden for many days after the entire cure of the disorder."* In addition to the articles mentioned in the latter part of this quotation, the patient may be allowed to eat moderately of ripe fruit; the pulp of roasted apples is very good; milk and bread may also be allowed him. There is one circumstance, however, which ought always to be attended to during convalescence, which is, to keep the bowels open; if this does not happen spontaneously, it must be effected by the use of laxatives; this is highly important, as it lessens greatly the danger of a relapse. Armstrong says, that he has seldom seen relapses in inflammatory diseases, when the bowels have been kept regular, and the antiphlogistic diet observed.

We have now finished what we have to say on the treatment of this form of typhus, and, from a review of it, it will appear, that all the remedies mentioned concur in the same tendency, i. e. to reduce the action of the heart and arteries, and to remove local inflammation when present. This course of treatment is the necessary and unavoidable result of the opinions we have adopted in relation to the pathology of typhus; if these are correct, if the disease is really one of excitement, and often of inflammation of some vital organ, then it follows, that the antiphlogistic plan of treatment, differently modified according to the grade of the fever, is the

* *Maladies de Gens de Mer.* vol. 1, p. 345.

proper one. Whether, however, the views of this disease, inculcated by the great Sydenham, proclaimed and defended in modern times by Armstrong and others, and embraced by the humble author of this essay, be true or not, time and the increasing light of the science must decide. The great object in our profession certainly is, to ascertain the true character of disease. If our pathological views are correct, then the practice legitimately deducible therefrom must be correct. It is one of the maxims of the great father of medicine, that he who knows the nature of a disease knows how to cure it, or, as Celsus elegantly expresses it, "*Estimatio causæ sæpe solvit morbum;*" and all our endeavors should be directed to this point. Until this be accomplished, our practice must partake somewhat of empiricism.

Treatment of Congestive Typhus. It has been already mentioned, that in this form of fever, the system does not react from the first stage of oppression, but continues in this state, in a greater or less degree, according to the violence of the cause producing it. In some cases the prostration is complete; in others, there is a partial and transitory reaction. From the appearances of great debility which present themselves in the commencement of congestive typhus, it has been customary to treat it by the administration of cordials and stimulants; this treatment would be proper enough if the debility was real; but if it is only apparent, if it be the result of that change in the circulation which has been noticed in a former part of this essay, and that (in the language of Armstrong) "it is no more to be accounted positive exhaustion than the loss of muscular power, which precedes and accompanies the treating of genuine apoplexy, to which, in fact, this modification of typhus has often a most forcible resemblance;" then it demands a different treatment. If, then, cordials and stimulants are not to be relied on for the cure of this form of typhus, (though we acknowledge that they are occasionally very useful adjuvants to the other means,) what is the proper plan of treatment? We answer, that our first endeavor should be to remove some portion of that blood, which is, (if we may use the expression,) suffocating the heart's action, by abstracting blood from the system. Under such circumstances we frequently succeed in relieving the heart, in some measure, of its overwhelming load of blood, and, as a necessary consequence of this, raising the pulse; this appears very paradoxical, but it is nevertheless true. Sydenham gives a remarkable instance of this effect of bleeding upon the pulse. In the case of a young man, whose skin was so cool that he could not persuade the attendants

he had a fever, and who appeared to be almost expiring, he drew blood freely, with the effect of raising the pulse to such a degree, that it required three or four subsequent bleedings to subdue it: bleeding, however, does not always have this effect; it sometimes weakens instead of raising the pulse. Its producing or not this latter effect is the test of its utility. If the pulse becomes more free and full under the loss of blood, then we may be certain that it is doing good; if, on the other hand, it renders it more feeble and weak, we may be equally assured that it is doing injury. We should therefore always keep our finger on the pulse, and be guided in the quantity to be drawn, by its effect on the beat of the artery; if it should become more full and expanded, we may suffer it to flow; if the reverse, it should be arrested immediately. It is often, however, extremely difficult to draw blood in congestive fever; "the acting of the heart (says Armstrong) is often so much overpowered in the first instance, that the blood merely trickles, or rather oozes, from the punctured vessel, for a considerable time, being much darker and thicker than natural; yet when a few ounces have been drawn, it usually flows with freedom, and becomes finally of a brighter color. Occasionally I have stood over a patient nearly half an hour before a stream of blood could be obtained from a vein, but at last it gushed out in a full current, and was not as easily restrained as in ordinary cases. Many a life (continues he) might be lost by binding up the arm too hastily, and, therefore, the operator should persevere, until he knows whether enough blood can be obtained." If we cannot, however, procure blood from a vein, we must resort to arteriotomy.

The manner of drawing the blood is a matter of some importance; it should not be abstracted suddenly and by a large orifice, but gradually;* by proceeding in this way the heart

* Dr. Rush forcibly illustrates the propriety of gradually abstracting blood, in certain states of malignant fever, by comparing the system to "a man struggling beneath a load of two hundred weight, who is able to lift but one hundred and seventy-five. In order to assist him (says Rush) it will be to no purpose to attempt to infuse additional vigor into his muscles by the use of a whip, or of strong drink. Every exertion will serve only to waste his strength. In this situation (supposing it impossible to divide the weight which confines him to the ground) let the pockets of this man be emptied of their contents, and let him be stripped of so much of his clothing as to reduce his weight five and twenty or thirty pounds. In this situation he will rise from the ground; but if the weights be abstracted suddenly, while he is in the act of exertion, he will rise with a spring that will endanger a second fall, and probably produce a temporary convulsion in his system. By abstracting the weights from his body more gradually, he will

much more certainly recovers its power. When the action of the heart and arteries have become more free, we ought then to arrest the flow of blood, as any further loss might again render this action weak, and place the patient in a worse situation than he was before the bleeding. "When both the pulse and the general circulation become manifestly freer, (after bleeding,) with a warm moist skin, tepid diluents should be the only beverage recommended; yet, when it still remains oppressed, and the tide of the circulation does not return to the surface, and more especially if blood has been freely drawn, some wine with warm water should be occasionally exhibited, and the patient speedily immersed in a bath strongly impregnated with salt, and at least about the temperature of 100° Fahrenheit's scale. He should remain in the bath till his skin becomes warm, and, on being removed, it should be well rubbed all over with hot flannels, and he ought then to be laid in an aired bed, with bottles of warm water at his feet. This plan, together with tepid wine and water occasionally, will often promote a flow of blood towards the skin, and relieve the viscera from congestion."*

But we are not to rely solely on bleeding, the bath, &c. in the cure of congestive fever. After bleeding we should immediately order large stimulating enemata, that the bowels may, if possible, be at once evacuated; and then commence the use of cathartics. Among them calomel is beyond all comparison the best; this given alone, or in combination with jalap, in doses sufficient to act freely on the bowels, will often prove of immense utility. One of the first triumphs of the calomel and jalap practice, instituted by the great Rush in the yellow fever of 1793, was the recovery of an apparently hopeless and desperate case of congestive fever. "The patient (says he) had passed twelve hours, before I began to give him this medicine, without a pulse, and with a cold sweat on all his limbs. His relations had given him over, and one of his neighbors complained to me of my neglecting to advise them to make immediate preparation for his funeral. But in this situation I did not despair of his recovery. Dr. Mitchell's account of the effect of purging in raising the pulse, excited a hope that he might be saved, provided his bowels could be opened. I now committed the exhibition of the purging medicine to Mr. Stall, one of my pupils, who mixed it and gave it with his own hands, three or four times

rise by degrees from the ground, and the system will accommodate itself in such a manner to the diminution of its pressure, as to resume its erect form, &c."

* Armstrong on Typhus.

a day. At length it operated and produced two copious fetid stools. His pulse rose immediately afterwards, and an universal moisture on his skin succeeded the cold sweat on his limbs. In a few days (continues he) he was out of danger, and soon afterwards appeared in the streets in good health, as the first fruits of the efficacy of mercurial purges in yellow fever." We have copied the whole of this case, because of its very instructive and interesting character. It evinces in a striking degree the effect evacuations from the bowels have in raising the pulse, and it should also teach that no case, however bad in appearance, should be abandoned as incurable: our motto indeed should be *nil desperandum*.

The bowels, in congestive fever, are often extremely torpid, and require the most powerful doses to rouse them to action. This is one of the cases in which we should disregard the injunctions of our materia medicas, and exhibit medicine in doses commensurate with the violence and obstinacy of the disease.

Armstrong has great confidence in the salutatory effects of calomel. We must confess that we believe it never does good in this way; the calomel, before it affects the mouth, has already won the victory over the disease. The reason why salivation has been thought to put an end to fever, is the generally simultaneous appearance of ptyalism and the cessation of the fever; but we are fully convinced that the former is the result of the latter, and not that the latter is produced by the former. Salivation, therefore, may generally be considered as a good sign, as it is a proof that the fever is either entirely subdued or very much on the wane.

Our great confidence, therefore, in calomel is from its superior efficacy in evacuating dark or green matter from the bowels; and in this only we believe consists its beneficial effect in fever.

The only local remedy which we have found very useful in cases of congestive fever, is the application of mustard poultices or sinapisms to the wrists, ancles, and pit of the stomach. When the surface of the body is cool and the pulse very weak and low, these sometimes produce an excellent effect; they are, particularly in such cases, greatly preferable to blisters.

Causes of Typhus Fever. We come now to a consideration of the causes of typhus. This is a subject on which great diversity of opinion exists even at the present day. While some are of opinion that it may arise from the putrefaction of animal matter, or from human effluvia, the result of crowding healthy persons into confined and ill ventilated places; others contend that it arises exclusively from, and is propagated by, a specific con-

tagion; and there are others still who deny to it any such property, and consider it merely as a variety of the common remittent and continued fever, and, of course, as arising from the same causes.

We shall examine these different opinions in succession, with as much fairness and impartiality as we can; and first, of that opinion which attributes the origin of typhus fever to animal putrefaction. This has for a long time been considered not only as a cause of the fever now under consideration, but of other fevers also. Pringle remarks, that "The hospital and jail fever are to be considered as the same disease, and little, if at all, different from such as have arisen after battles, when the bodies of the slain have been left unburied to rot upon the field. This, Galen (continues Pringle) notes as one of the causes of pestilential fevers."* There is, however, such an abundance of evidence in refutation of this opinion, as "to leave no loop or hinge to hang a doubt on." Bancroft, in his able and philosophical work on fevers, has collected a mass of testimony to this effect, some of which we shall take the liberty to copy into this essay. The most remarkable fact which he relates, is the exhumation made in the churchyard of the Saints Innocens, at Paris, in the year 1786. "The churchyard of the Saints Innocens, at Paris, situated in one of the most populous quarters of the city, had been made the depository of so many bodies, that, although its area enclosed more than 1700 square toises, or near two acres, yet the soil had been raised by them eight or ten feet higher than the level of the adjoining streets; and upon the most moderate calculation, considerably more than six hundred thousand bodies had been buried in it, during the last six centuries; previous to which date, it was already a very ancient burial ground. Numerous complaints having been made concerning the offensive smells, which arose from this spot, and sometimes penetrated into the adjoining houses, and the public mind being greatly alarmed, it was at last determined to forbid all future burials there, and to remove so much of the superstratum as would reduce the surface to the level of the streets. This work was undertaken in 1786, under the superintendence of M. Thouret, a physician of eminence in Paris, and in two years he accomplished the removal of that superstratum, almost the whole of which was impregnated, or *infected*, as M. Thouret styles it, with the remains of carcases, and of quantities of filth and ordure, thrown upon it from the adjoining houses."

* Observations on Jail or Hospital Fever, p. 320.

"The exhumations" (says M. Thouret) "were principally executed during the winter; but a considerable part of them was also carried on during the *greatest heats* of summer. They were begun with every possible care, and with every known precaution; but they were afterwards continued, almost for the *whole period* of the operations, without employing, it may be said, *any precaution whatever*; yet no danger manifested itself in the whole course of our labors—no accident occurred to disturb the public tranquility!"*

The philanthropic John Howard, in his work on Lazarettos, p. 25, remarks, that "The governor, at the French hospital at Smyrna, told me, that in the last dreadful plague there, his house was rendered almost intolerable by an offensive scent, especially if he opened any of those windows which looked toward the great burying ground, where numbers were left, every day, unburied; but that it had no effect on the health of himself or his family. An opulent merchant, in this city, adds he, likewise told me that he and his family had felt the same inconvenience, without any bad consequences."†

In the Edinburgh Medical and Surgical Journal, of October, 1810, is an account, given by Dr. Chisholm, of a manufactory at *Conham*, near Bristol, destined for the conversion of animal flesh into a substance resembling spermaceti, by cutting up dead horses, asses, dogs, &c. and putting their muscular parts into boxes with holes for the admission of water, and afterwards placing them in pits filled with water; while the entrails and useless parts of many hundreds of carcasses, were left to putrefy on the surface of the ground. The effluvia of these putrefying animal matters were highly offensive to the overseer of this manufactory, and to the workmen employed under him, as well as to others within their

* "It does not appear, after the fullest enquiry, that any febrile disorder was ever produced by this immense mass of corruption, during the removals made in 1786, or while it was suffered to remain as a burying ground. The grave diggers were, indeed, sometimes thrown down suddenly, and, for a time, deprived of sense and motion, by the concentrated vapors which escaped, upon accidentally breaking open, by their spades, the abdominal viscera of bodies, in an early stage of putrefaction. These vapors, also, in a more diffused state, are said to have produced nausea, loss of appetite, and, in a course of years, paleness of countenance, debility, tremors, &c. But fever of any kind, and much less *contagious fever*, does not appear to have been noticed, as resulting from the offensive or putrid matters of this churchyard, either to the grave diggers or to the neighboring inhabitants." Bancroft, pages 93, 94, 95.

† Bancroft, p. 97.



reach, yet no injury was done to the health of any person during the two years in which these operations were continued.”*

In addition to these strong facts, and many others equally so, which we deem it unnecessary to mention, we may add that in no one instance that we have heard of, has typhus fever been attributed to the concentrated and putrid effluvia arising from the numerous dead bodies in the dissecting rooms attached to the different medical colleges in this and other countries. These facts are considered sufficient to prove that fever of no kind or description ever arises from animal putrefaction.

The next assertion, that typhus fever is produced by the effluvia arising from crowding healthy persons into close and ill ventilated places, is equally destitute of foundation. This is an almost universal opinion. We can hardly take up any work on the subject, in which we do not find this set down as one of the causes of typhus. Cullen, Chisholm, Good, and Wilson, all mention human effluvium as one of the causes of this fever. Cullen remarks, that “the effluvia constantly arising from the human body, if long retained in the same place without being diffused in the atmosphere, acquire a singular virulence; and in that state being applied to the bodies of men, they become the cause of a fever which is highly contagious.” Chisholm says, “the cause, in fact, of *typhus*, is, I believe, an undefined change in the atmospheric air, brought about by its confinement in a very limited space, and incapacity, in a great degree, of renewal, and the respiration of an effluvium, emanating from the persons inhabiting the wretched close dwellings in which fever is found.”† Good, when speaking of the causes of typhus, remarks, that “its common cause is febrile miasm, issuing from the decomposition of human effluvium, under the influence of the ordinary auxiliaries of a close and stagnant atmosphere.”‡ Wilson, likewise, says, that “the effluvia of the living body, become putrid by stagnation, are capable of producing it,”|| i. e. typhus fever. We see, therefore, that the ablest writers on medicine have adopted and published this opinion; and when bolstered up by such authority, it is not surprizing that it should have become so general. Bancroft, however, by a series of the most undoubted and convincing facts, has completely overturned this opinion, and has proved, beyond cavil or doubt, that it is never productive of typhus fever. We will select the most striking of the facts recorded on this subject.

* Bancroft, p. 442.

† Ibid, p. 99.

‡ Study of Medicine, vol. 2, p. 124.

|| On Fevers, vol. 1, p. 154.

"The people of the island of Oonalaska inhabit jouts, or subterraneous dwellings, each common to many families, in which they live in horrible filthiness." (Pennant's Arctic Zoology, vol. 1.) "And the Samoiedes live in subterraneous dwellings, equally filthy, for almost nine months in the year, who are yet reported by travellers to be strong, active, and healthy. In addition to all this filth, crowding, and want of ventilation, the *food* of these people may be considered as little better than putrefaction itself." Mr. Pennant, describing that of the natives of Kamschatka, says, "their ambrosial repast is the huigal, or fish flung into a pit, until it is *quite rotten*, when it is served up in a state of carrion, and with a stench that is insupportable to every nose but that of a Kam-schatkan.* These people, notwithstanding their mode of living, do, with the exception of the scurvy, enjoy good health.

"The Greenlanders and Esquimaux appear, by the accounts of those celebrated navigators, Davis, Frobisher, Baffin, Henry Ellis, &c., as well as Bishop, Egede, and Crantz, to live during the greater part of the year in very close, ill ventilated, and crowded habitations, (without chimneys,) which, notwithstanding the great severity of the cold, they keep extremely warm by their numbers and breath, assisted by a single burning lamp in each, and by excluding fresh air so completely, that any other people would think themselves in danger of being suffocated by the offensive vapors continually exhaling from the lungs and bodies of the inhabitants, and which involve them as a thick fog; and yet fever of any kind is a rare disease among these people, though, like those of Kamschatka, &c. they are much disposed to scurvy."†

Dr. Mathew Guthrie, physician at St. Petersburg, in a letter to Dr. Priestly, mentions, that "the Russian boor lives in a wooden house, caulked with moss, so as to be snug and *close*. It is furnished with an oven which answers the triple purpose of heating the house, dressing the victuals, and supporting on its flat top the greasy matrass on which he and his wife lie."

"During the long severe winter season, the cold prevents them from airing this habitation, so that the air cannot be very pure, considering that four, five, or six people eat and sleep in one room, and undergo, during the night, a *most stewing process* from the heat and closeness of their situation, insomuch that they have the appearance of being dipped in water, and raise a steam and smell in the room, not offensive

* Bancroft, p. 100.

† Ibid, p. 101.

to themselves, but *scarcely supportable* to the person whom curiosity may lead thither."

"Now, if it be considered that this *human effluvium* must adhere to every thing in the room, especially to the sheep skins or mattrass on which they sleep, the moss in the walls, &c., and that the apartment is never ventilated for six months, at least; at the same time that these people are living upon salt fish, and the whole time without fresh vegetables, &c.—if it be a fact that they are, in spite of all these predisposing causes, *strangers to putrid disease*, it will sufficiently justify my first assertion, that the regimen nature has dictated to these people, is highly antiseptic."

Bancroft remarks, that "Dr. Guthrie has stated these facts principally to show the supposed beneficial effects of the Russian drink called quass, &c.; but I am entitled to avail myself of them for the purpose of demonstrating that long confinement in close unventilated houses, without chimneys, in an atmosphere replete with human effluvia, and in very cold weather, when typhus or contagious fever is commonly most prevalent, does not produce that disease, it being, as will hereafter appear, unknown in that part of the world."* From these facts we learn that the effluvia arising from the crowding of healthy persons into close and unventilated situations, in cold countries, at least, is not productive of fever; and upon examination, we shall find that the same cause is equally innocuous as regards the production of typhus fever in warmer regions of the globe.

The African slave ships present striking examples of this fact. "The poor wretches (says Lind, in his treatise on jail distemper) are crowded together below the deck, as close as they possibly can lie, with only a small separation between the men and women; every night they are shut up under close hatches, in a sultry climate, barred down with iron to prevent an insurrection; and though some have been suffocated by the close confinement, or foul air, though they are subject to the flux, and suffer from a change of climate, yet an infection is scarce known among them; or if an *accidental* fever, occurring from the change of climate, should become infectious, it is generally much more mild than in the opposite situation," i. e. that of ragged felons under transportation. Bancroft remarking upon this statement, says, "that Dr. Lind, influenced, as he was, by the commonly received opinions, mentions an infection (meaning fever) as being '*scarce known*' in the slave ships, instead of asserting, as he might

* Bancroft, p. 102.

have done with truth, that it is *never known* ; for, (continues Bancroft,) after very extensive enquiries, I am fully convinced that fever of any kind rarely occurs on board these vessels, and *contagious fever never*."

A striking instance of the harmlessness of the effluvia arising from the excessive crowding of persons into confined and close situations, may be found in the "Narrative of the deportation to Cayenne," of J. J. Job. Aimé, and one hundred and ninety-two other persons, on board the *Decade* frigate, in consequence of the revolution (in France) of the 18th Fructidor, (Sept. 4,) 1797, written by himself, and printed in 1800. In this narrative the writer says, "we were placed in the between decks, before the fore mast and main mast, occupying nearly one fourth of the superficies of the vessel, having about four feet and a half in height, and receiving no light but by the scuttles; that is to say, by two openings of three feet square."

"In this place, the door of which was locked, were crowded and squeezed together one hundred and ninety-three individuals, mostly aged and infirm. We lay in two rows, one over the other, forming, as it were, two stories, in hammocks of coarse cloth, and extremely narrow. Those below could not raise their heads without hitting those above; neither could any of us make the smallest motion without disturbing his neighbors; for we all touched each other, and not having the least spare room, formed, as it were, but one mass. And that nothing might be wanting to increase the horror of our situation, as we were not permitted to go out for fourteen hours together, (i. e. from 6 P. M., until 7½ A. M.,) and sometimes more, tubs had been placed in the midst of us, where we might satisfy the indispensable wants of nature; and to get to these sorry receptacles, we were obliged to creep on our bellies beneath the hammocks. How insupportable then must have been the infection of such a close confined place, which was already poisoned by our own exhalations! Indeed, the air which passed from this hole was so hot and fetid, that the sentinels placed at the hatchways as our guard, demanded that the time of their duty, at so dangerous a post, might be shortened."

In addition to all this, they "were condemned to subsist on the coarsest and the most disgusting half putrefied food," and "to endure the greatest and most offensive personal filth."* Yet, notwithstanding this combination of the most disgusting and distressing circumstances imaginable, nothing like conta-

* Bancroft, p. 105-6.

gious fever manifested itself, although they were kept in this state for ninety-six days. If "crowding, want of ventilation, filthy clothing, and unwholesome, corrupting food, together with anxiety and dejection of mind," could have generated the contagion of typhus, it ought to have been present here in its most concentrated state.

Dr. Lind, in his essay on preserving the health of seamen, says, that "the surgeon of the Panther (of sixty-four guns) told me, that forty of her men had died of the scurvy on their passage home," (from the West Indies,) "and that during that time there were usually ninety patients in the sick apartment. The place appropriated for the sick was in the bay of the ship, and had no pipe from the ventilator, nor any scuttles cut through its sides for the admission of the fresh air. A number of patients, thus closely crowded together, rendered the place so disagreeable and suffocating, that the sick were in a manner stifled for want of air. The surgeon, when visiting, could scarcely breathe in it, or remain for any length of time, without being obliged to have recourse often to the fresh air upon deck, and sometimes to spirits of harts-horn, or to a glass of wine, for his immediate relief. He observed, that both the virulence and mortality of the scurvy were heightened by the uncultivated air of the place in which the sick, for several weeks, had been confined; yet, out of above a hundred patients sent to the hospital by this surgeon, not one was remarked to have any symptom of contagion generated in that apartment." This case is the stronger in point, because the men thus crowded together were in a diseased state. Many more facts of this kind might be mentioned, but these are deemed amply sufficient to satisfy the most sceptical that human effluvium is not productive of contagious fever.

Of a Specific Contagion as the Cause of Typhus. The contagiousness or noncontagiousness of typhus fever, has been the source of much, and occasionally bitter, controversy, between the members of the medical profession. Until the present century, few or none were found so bold as to question its infectious character, and thus run counter to the prejudices, not only of the profession, but of society at large. Though, however, the doctrine of contagion, as an attribute of typhus, remained for a long time unimpugned; yet it finally came to be doubted, and the noncontagionists can now number as their friends a large proportion of the distinguished members of the profession. One reason of our long continued and implicit credence in the contagiousness of typhus, may be found in the very character of man. It is his nature

to cling with great tenacity to long fostered and traditional opinions, clothed as they are with the names and authority of the venerable sages of other times; and he feels a pride in preserving his opinions, when once formed, unchanged; and thus it is that the prejudices of education, and the pride of consistency of opinion, often blind us to the light of truth, and rivet upon us the chains of error.

As it is a subject of much importance in many points of view, we will enter into the merits of this question as fully as the length of this essay will admit. The vexations, embarrassments, and restrictions upon commercial intercourse, produced by quarantine regulations, and the large and expensive establishments* in different countries for carrying them into operation, and the delay and inconvenience to individuals, all demand that this question should be rigidly examined into, and settled finally one way or the other. If, upon strict scrutiny, it should be found not to be contagious, then the abolition of quarantine establishments ought immediately to follow. If, however, after the most thorough and impartial examination, it should appear to be contagious, the quarantine regulations cannot be too rigidly enforced.

In discussing this subject, we shall in the first place contend that typhus fever has not the characteristics of a contagious disease; 2d, that it often has occurred without the presence of contagion; and 3d, that it often evinces no contagious character whatever. 1st. Typhus fever has not the characteristics of a contagious disease. All known contagious diseases run a regular and determinate course, and are marked by symptoms which unequivocally distinguish them from other diseases. Thus, smallpox, measles, chickenpox, and cowpox have each peculiar symptoms, which succeed each other in regular succession, and pursue an invariable and certain course, independently of the treatment used. Thus, smallpox, for instance, though it may have its symptoms ameliorated by a proper treatment, or the reverse may take place by improper management, will, nevertheless, pass through all its regular and determinate stages. The same may be said of the other examples adduced. Now, the question may be asked, does typhus fever come up to this rule? We answer in the negative. That it has no distinctly marked pathognomonic symptoms, is evident from the fact, that dif-

* Maclean, in his work on Epidemics, vol. 1, p. 456, says, "In this country (i. e. England) they have cost the nation for the last fifteen years, independently of the detention of ships, crews, passengers, and merchandize, at least £300,000."

ferent appellations are frequently given to the same fever by different physicians. Thus, one will pronounce it to be the common continued fever, another the genuine typhus, and a third, meeting with an unusually violent case, would call it yellow fever;* and there is not a single symptom laid down as belonging to typhus, which is not to be met with in other fevers.

Neither does it, like known contagious diseases, run a defined and determinate course. It may last one, two, three, or six weeks, according to the treatment pursued. Again, the period at which morbid action is developed after the reception of contagion, is determinate in contagious diseases. This is not the case in typhus fever, as contagionists say that the infection may lie dormant during a period extending from one day to six months.†

In the next place, acknowledged contagious diseases affect a person but once during his life; this is a wise and beneficent law stamped upon them by the great author of creation to prevent them from becoming universally prevalent. "It is the incapability of affecting the same person more than once which, in general contagious diseases, sets boundaries to infection; and could alone, where no precautions are taken, prevent communities from being extinguished."‡ Typhus fever does not possess this characteristic; it may, and frequently does, attack the same person repeatedly, and it is obvious that possessing, as it unquestionably does, this attribute, if you invest it also with the power of producing and propagating itself by a specified contagion, no human power

* Dr. Carrathers, surgeon of the Malta, in his account of the typhus which prevailed on board that vessel, and which is to be found in Trotter's "*Medicina Nautica*," remarks, that "many of the sick had yellowness of the skin, insomuch that he suspected that the yellow fever had been introduced by communication with a sickly ship from the West Indies!" See Cooke's *Practice*, vol. 1, p. 497.

† Bancroft says the fever may make its appearance six months after the infection, and Haygarth expresses the same opinion. We cannot help applying a remark of Dr. Armstrong's to the opinions of B. and H. on this subject. He says, "If an author is persuaded that there must be a specific contagion as the cause of every fever resembling typhus, he will undoubtedly go far to seek it." The above named gentlemen prove the truth of this remark, for they, with the most wonderful acuteness and sagacity, pretend to have detected its operation six months after exposure to it. Unlike the bed of Procrustes, which reduces every thing to the same common size, they endow contagion with the wonderful property of expanding or contracting, so as to embrace every case which may occur from one day to six months after its application. Does this diversity in the time of producing its effects, comport with our ideas of something specific and unchangeable in its nature? Do the known contagions ever operate in this way?

‡ Maclean on *Epidemics*, vol. 1, p. 100.

could prevent its spreading almost over the whole human family. Every person in health would be attacked, as often as he came within the infectious distance of any one laboring under the disease. Those only would escape who could preserve themselves constantly beyond that distance, or were laboring under some malady higher, in degree, than the prevailing epidemic. But, as disease spread, no person in health could continue long beyond the sphere of infection; and those who had been protected by diseases higher in degree, would, upon recovering, become liable to be affected by the contagion. It would observe a geometrical progression, diverging, as it were, from the centre to every point of the circumference of a city, a camp, an hospital, or a ship, while it had subjects to operate upon; i. e. while a single individual of the community remained alive it would never cease. Those who recovered would, again and again, be seized; no person who remained within the pale of society could escape. The malady would be communicated to the most distant nations. Infection would proceed in a continued circle, until the whole human race was extinguished.”* This extract is a highly wrought, though true, account of what would be the effect of adding to the powers which typhus has of repeatedly attacking the same person, that of being capable of generating and propagating itself by contagion. Inasmuch, therefore, as typhus fever has no regular and pathognomonic symptoms, and as the period at which the symptoms of disease are manifested, after infection, varies, according to contagionists, from one day to six months, which is inconsistent with our ideas of something specific in its character, and uniform in its operation; and as it is unlike all known contagious diseases, in being capable of affecting the same person repeatedly, and does not spread until it becomes universal, which should be the inevitable effect of uniting to this latter trait the property of contagion, we think, therefore, it must be admitted that it does not possess the characteristics of a contagious disease.

2d. That it has often occurred without the presence of contagion. To prove this second proposition, we will adduce facts mentioned by contagionists themselves. It is presumed that as the advocates for contagion would not gratuitously and without good reason mention facts subversive of their own opinions, that, therefore, these facts ought to have more weight than would be attached to them under other circumstances; as a man is not to be suspected of insincerity or im-

* Maclean on Epidemics, vol. 1, p. 214-15.

proper bias when he gives evidence against himself. But to the testimony.

Trotter, who was a zealous advocate for the contagious origin of typhus, mentions, in his *Medicina Nautica*, many instances of it, which could not be traced to contagion. On board the *Niger*, he says, "It first appeared among the marines, but latterly affected the seamen. Its origin could not be exactly traced."* In the *Glory*, also, "We could not trace it distinctly to its source."† "No imported contagion was duly authenticated"‡ as the cause of the fever which broke out on board the *Edgar*, while at sea. Of that on board the *Pompey*, at sea, "No satisfactory account could be obtained of its origin."|| Trotter says, Mr. Johnson remarks in relation to the fever which occurred on board the *Saturn*, "I could not trace this fever to any infection imported by any of the people."§ In the *Magnificent*, the surgeon says, the fever spread more than he had expected; "but still I cannot trace it satisfactorily to a contagious cause."¶ Trotter says, when speaking of the typhus on board the *Captain*, "No imported contagion has been suspected here."** Of the same on board the *Uranic*, he says, "It does not appear that any imported contagion has been suspected as the cause of this fever."†† Speaking of the same vessel again, he says, "It does not appear that any imported contagion could be suspected here." In giving the result of his experience for many years, he says, "It is very certain that this fever is generally spread by contagion; but it is equally certain, that it frequently arises in places where there could be no suspicion of communication with infected persons or clothing."‡‡ The above facts are taken from a treatise on pathology and therapeutics, by John E. Cooke, M. D., professor, of Transylvania University, in which this subject is discussed with the most masterly ability. I shall mention but one more case of this kind, to be found in Assalini's work on the Plague. He says, that in the years 1799 and 1800, "An epidemic disease spread its ravages in the hospitals of the army of Italy, and carried off daily its numerous victims." The fever spreading rapidly, and much alarm being created, "the public authority (says he) thought proper to consult the school of medicine of Montpellier, which hastened to calm their inquietude, and reanimate their drooping spirits, by proving that this fever, falsely regarded as pestilential, was not at all dif-

* *Medicina Nautica*, vol. 1, p. 153. † *Ibid*, p. 160. ‡ *Ibid*, vol. 3, p. 63.

|| *Ibid*, p. 64.

§ *Ibid*, p. 147.

¶ *Ibid*, p. 167.

** *Ibid*, p. 60.

†† *Ibid*, p. 151.

‡‡ *Ibid*, vol. 1, 252.

ferent from the fever of hospitals, the *typhus carcerum* of Pringle, or the fevers of camps and armies, *febris castrensis*." "With regard to this disease, the majority attributed it to rains and fogs. A citizen of the environs of Antibes assured me, that if this disease had taken place some months before, it would, without fail, have been attributed to the vessels just arrived from Egypt, and which had put ashore at Fre-gus without performing quarantine."* These facts are deemed sufficient to prove that typhus often appears without the least reason to suspect contagion as its cause.

Physicians, however, aware of this fact, and unwilling to surrender their long cherished opinions, are driven to the absurd alternative of supposing that typhus may have a double origin, or, in other words, that the specific contagion of typhus may be produced by other causes than itself. Thus, Gregory says, "there is nothing improbable in the supposition, that what originated in cold may be afterwards propagated by contagion."† Dr. Adams, in his work on morbid poisons, inculcates the same opinion. To these, and all similar opinions, we will apply the language of Bancroft when alluding to that of Dr. Adams on this subject. He says, "that Dr. Adams, who is accustomed and qualified to reason, should have believed any thing so unphilosophical and *incongruous*, would have been incomprehensible to me, if so many others had not discarded common sense on the subject of contagion. To represent a disease which is *notoriously contagious*, and propagated by contagion, as capable of being produced by other, and those very different, means, is to multiply causes unnecessarily, and, therefore, unjustifiably; and it moreover destroys the natural and just influence of causes upon their effects, by making the same disease result from very dissimilar causes; and, a little farther on, he observes, that "were it possible for typhus thus frequently and easily to originate without contagion, and at the same time acquire and multiply itself by a contagious quality, who could ever hope to escape the disease."‡

3d. That it often evinces no contagious character whatever. Contagionists themselves afford evidence to this effect. Trotter, in his *Medicina Nautica*, mentions many instances in which it was not contagious. "A fever (says he) of the typhus kind appeared among the gentlemen of the *Invincible*, in Torbay, which extended to a number of cases.

* Assalini on the Plague, p. 89, 90, 91.

† Gregory's Practice of Physic, vol. 1, p. 55.

‡ Bancroft on Fevers, p. 310, 311.

Yet it is singular, that it did not affect any of the seamen, although some of them attended their officer, who slept in the gun room, and where communication with the ship's company was not prevented."* "The Niger has been ten weeks at sea; at leaving port four relapses in fever happened among men discharged from the Warlar. Mr. Burd does not assign any cause for these returns; no infection spread from them."†

"We know of nothing (says Trotter) that can propagate this fever, but exhalations from the body. But a patient in typhus was sent from the Venerable to the hospital ship, with a fætor about him, that exceeded every thing of the kind which ever came within my knowledge. After being washed and shifted, it still continued, and was perceived at a considerable distance. He died in a few days; yet nobody was infected from him, either in his own ship, or our hospital."‡

Speaking of the Charon hospital ship, Trotter says, "We had in that hospital many malignant cases of typhus, and some deaths, yet no infection was ever spread there."||

In 1790, a number of troops embarked on board the Gorgon to go to New South Wales. While lying in the harbor of Portsmouth, typhus fever prevailed among the soldiers, and several died. "The troops lived on the lower deck, and when the ports were shut at night, the exhalations from below naturally ascended through the gratings among the sailors; not one of them had the slightest sign of infection."§ The above facts are also derived from Cooke's work on pathology and therapeutics.

In the instance already cited from Assalini, of the fever which attacked the army of Italy, in the Ligurian republic, it is stated in a report made to the society of the school of medicine at Paris, that "it is was of the nature of jail fever, and was not at all contagious."¶

Wilson, in a note to page 156 of his chapter on continued fevers, says, "It is remarkable, indeed, that we sometimes meet with malignant fevers, which do not appear to be at all contagious." "Sometimes, (says he,) Dr. Lind observes, one man may be seized with the petechial or with yellow fever, while the rest continue unaffected." Dr. Lind gives several instances in support of this observation.

These facts prove that typhus fever is not, at least, always

* Trotter's *Medicina Nautica*, vol. 1, p. 109.

† Ibid, p. 195.

|| Ibid, p. 179.

¶ Assalini on the Plague.

‡ Ibid, p. 164.

§ Ibid, p. 210.

contagious, and it is inconsistent with our ideas of a strictly contagious disease, that it should at one time possess this characteristic, and at another be destitute of it. Known contagious diseases are always and invariably the same; they are contagious under all circumstances; but typhus fever has been shown not to be so; and, to use the language of Bancroft, "as the same disease, in suitable circumstances, is *always*, or *never*, contagious, we may safely rely" on the numerous facts adduced, to disprove the assertion, that contagion is one of the characteristics of typhus.

The advocates, however, for the contagious nature of typhus, in order to avoid the inference legitimately deducible from the above facts, and others which exist, are driven to the necessity of reducing the question of its contagious character to very narrow limits. They admit that it does not evince this quality in situations where there is a free circulation of pure atmosphere;* but contend that it possesses it in the impure atmosphere of close, filthy, and unventilated apartments. The fact of its attacking many persons in succession under such circumstances, has been assumed as sufficient ground for a belief in its contagious character; but this surely only proves the existence of some general cause acting on all in those circumstances, and not that the disease is communicated from one to the other by means of a specific contagion. That general cause is unquestionably the impure atmosphere itself; for, if an individual laboring under typhus is removed out of the foul air, he does not propagate the disease; but the impure atmosphere will continue to produce it in most of those subjected to its influence. The circumstances, then, under which alone contagionists say that typhus is infectious, being of themselves capable of producing it, or exciting it, are in them already predisposed; but it is unnecessary and unphilosophical to assign that as a cause of the fever, which is acknowledged to be only operative during the presence of causes which, without this, are capable of producing it. We do not think, therefore, that contagionists can sustain themselves on this ground. Under the next head of this essay we will speak more particularly of the cause of this impure atmosphere, which we consider as alone capable of producing this fever.

We have now finished what we have to say on this part

* It was from a knowledge and conviction of this fact, that infirmaries and houses of recovery were established in England. These institutions for the reception of the wretched paupers of that country, were the result of the philanthropic labors of Ferriar, Percival, and others; and will constitute a lasting monument to their memory and reputation.

of the subject. We trust that we have established to the satisfaction of every unprejudiced mind, the three positions for which we have contended. 1st, that typhus fever has not the characteristics of a contagious disease; 2d, that it has often occurred without the presence of contagion; and 3d, that it often evinces no contagious character whatever. If we have even proved the last of our positions, then Bancroft and his disciples at least ought to admit the justice of our conclusion that it is never contagious, as he uses the same train of arguments against the contagious nature of yellow fever, and comes to the same conclusion in regard to it, from the facts he had collected upon that subject.

There are, it is true, some apparently strong facts on record in proof of its infectious nature, but we believe that they cannot stand the test of rigid scrutiny. The fevers which occurred at the black assize, at Oxford, in 1577, and at the Old Bailey, in 1750, are always held up as evidences of its contagious character; but no one can read the remarks of Bancroft upon them, without being convinced of their utter want of claim to be considered in this light. To notice all the instances of this kind to be found in books, would swell this essay far beyond reasonable limits, and we shall therefore not attempt it.

We will now, in the last place, examine the opinion of those who deny that typhus fever possesses the property of contagion, and who consider it as a mere variety of the continued and remittent fever, and of course as arising from the same causes.

We are among those who entertain this opinion; and from a close and impartial examination of all the facts and arguments bearing on the question, are perfectly satisfied of its correctness. In order to render this subject as clear as possible, we will in the first place endeavor to show "that typhus fever arises in situations in which miasmata abound." 2d. "That the symptoms of typhus are those of autumnal or miasmatic fevers!" 3d. "That typhus fever appears in company with miasmatic diseases, and that they are mutually convertible into one another."* Before proceeding, however, to a consideration of the different heads of this part of the subject, it will be just and proper to remark, that we shall stand indebted to Dr. Cooke's admirable essay on the causes of typhus, contained in the first volume of his work on pathology and therapeutics, for most of the facts to be mentioned on this subject. The author of the above mentioned work

* Cooke's Pathology and Therapeutics, vol. 1, p. 450.

has so completely covered the whole ground of controversy, and so perfectly exhausted the subject, that but little else is left for others to do, than merely to draw upon the rich storehouse of facts, collected by his unceasing and indefatigable industry.

1st. Typhus fever arises in situations where miasmata abound. Currie mentions, "that the corporation of Liverpool, being about to apply to parliament for powers to improve the streets and police of the town, requested the physicians of the Infirmary and Dispensary to suggest to them such alterations as might contribute to the health and comfort of the inhabitants.* The physicians took this request into serious consideration, and presented a report of considerable extent, including a view of *the causes* of the uncommon sickness of the two preceding years, and of *the measures requisite to prevent its recurrence*, and to remove the frequency of contagion in the habitations of the poor." The physicians consulted, and pointed out the necessity of enforcing cleanliness in the streets, to which end an improvement of the pavement was represented to be essential; and they particularly advised a general review of the common sewers, and an improvement of their structure, on the principles of the report on this particular subject, addressed by them to the mayor and magistrates, in 1788. They further advised, that effectual provision should be made for draining the grounds within the liberties, and particularly to the north of the town. "Repeated remonstrances (the words of the report) have been made for the last twenty years, on the collections of standing water, including filth of every kind, which are suffered to remain in the district which extends along the termination of the streets, from St. Paul's square to Byron street, and to which the low fevers, which in the autumnal months especially infect these streets, are principally to be imputed."† In this instance, the physicians, with a view to prevent the recurrence of the "low fevers" which had prevailed so widely during the two preceding years, recommend the removal of those circumstances, which are every where fruitful in the production of miasmatic exhalations.

At page 23, Currie mentions another fact, clearly evincing the miasmatic origin of typhus fever. He says—"The 30th regiment, as is usual with troops in Liverpool, was billeted in the town, but paraded and mounted guard in the fort, situated north of the town, on the banks of the river. The gene-

* Currie's Med. Reports, p. 246.

† Ibid, p. 247.

ral guard room had been used, previous to the arrival of the 30th, as a place of confinement for deserters: it was extremely close and dirty, and under it was a cellar, which in the winter had been full of water. This water was now half evaporated, and from the surface issued offensive exhalations." In a dark, narrow, and unventilated cell of the guard room, it was usual to confine such men as were sent to the guard for misbehavior; and about the 20th of May, 1792, several men had been shut up in this place on account of drunkenness, and suffered to remain there twenty-four hours, under the debility that succeeds intoxication. The typhus or jail fever made its appearance in two of these men about the first of June, and spread with great rapidity.* All the precautions used, by cleansing and ventilating the guard room, were ineffectual in preventing the progress of the fever. The weather was at this time wet, and extremely cold for the season: the men on guard could not be prevailed on to remain in the open air; and, from passing the night in the infected guard room, several of the privates of the successive reliefs caught the infection, and fell ill on the 10th, 11th, and 12th of the month.* We here find that all attempts at arresting the fever by washing and ventilation, were in vain; and why? because the source of the fever obviously was the "offensive exhalations" from the cellar under the guard room; and it was not until the guard room was shut up, and the soldiers removed to "a temporary shed erected in its stead," that the fever ceased.

Dr. Ferriar, of Manchester, in England, in his address to the police on the best means for the prevention of fevers, remarks, "that during the late epidemic, it was observed that the fever (i. e. typhus) prevailed most in streets which were not drained, or in which dunghills were suffered to accumulate, or where the blood and garbage from slaughter houses were allowed to stagnate." These facts, and many others of a similar import which we might mention, unquestionably prove that typhus fever arises on land, in situations where miasmata must abound.

It has also frequently appeared in ships at sea; and after a full and rigid examination of the many instances of the kind to be met with in books, we think we can venture to assert that in every instance, where the circumstances attending its origin and progress in such situations are detailed, they will be found abundantly sufficient to produce fever, without bringing in contagion to account for it. One

* Currie's Med. Reports, p. 25.

of two things will be generally found to exist; either that the state of the vessel as regards filth, &c. is such as would probably of itself give rise to febrific exhalation; or, that the crew has been exposed to miasmata on land, and being thus predisposed to fever, the exposure to bad weather, the inclemencies of a sea voyage, and the fatigue incident to such situations, act as existing causes, and fever is the result.

2d. That the symptoms of typhus are those of autumnal or miasmatic fevers. Under this head we shall say but little, because we believe that no one will question its truth, who examines into it with any degree of attention. Dr. Armstrong, in speaking of the identity of the remittent and typhus fevers, remarks, that the remittent and typhus fever from *malaria* or *marsh effluvium*, "has a combination of symptoms exactly similar to those which occur in continued typhus fever, and which, *as a combination*, occur in no two other affections whatever."* They both commence with a stage of oppression or weakened action of the heart; in both this first stage occasionally continues unchanged, and constitutes then the congestive fever; but in both the heart generally reacts from this first stage of oppression, and produces the open form of fever, the succeeding symptoms in each being dependent on the degree of this reaction, and the previous state of the different organs of the body.

The general symptoms in both are, therefore, alike, and if we descend to an examination of particular symptoms, we shall find none which exclusively belong to typhus. Petechiæ, which are so common in typhus, as to have given it a name, occur also in other fevers. Rush mentions their frequent appearance in the yellow fever of 1793, and almost every practitioner of any experience has often met with them in the autumnal fever of this country.

Yellowness of the skin is a frequent symptom of miasmatic fevers. It also often occurs in typhus fever. Mr. Caruthers, surgeon of the Malta, in his account of the typhus which had appeared in that vessel, says, that "many of the sick had yellowness of the skin, insomuch that he suspected that the yellow fever had been introduced by communication with a sickly ship from the West Indies."† In reference to the above statement, Trotter remarks that "Icterus is not an unfrequent symptom in typhus; it was particularly remarked among our seamen in the warm summer of 1794;"‡ and in vol. 1, p. 259, he says, "I have often seen the eyes and

* Medico-Chirurgical Review, September No., 1822, p. 395.

† Trotter's Medicina Nautica, vol. 3, p. 215.

‡ Ibid, p. 218.

skin, with the urine, as deeply tinged with bile in typhus, as I have remarked them in the yellow fever of the West Indies."

We will now proceed to the consideration of the third and last division of this part of our subject, which is, "That typhus fever appears in company with miasmatic diseases, and that they are mutually convertible into each other." To prove this we shall resort to the testimony of those who were perfectly familiar with the disease in all its different forms.

It is now generally acknowledged that dysentery is but a form of our autumnal bilious fever, and, of course, that it originates from the same cause, or, in other words, that it is, what the unnerring sagacity of Sydenham long since pronounced it, a "*febris introversa*." Typhus fever, then, often appears in company with this disease. Pringle, in his account of the diseases of the army when in Germany, in the year 1743, says, "The state of those at Feckenheim has been already mentioned; there the hospital fever and dysentery grew daily worse."*

Trotter says, the ship Reasonable arrived at Spithead in January, 1794, and "landed upwards of a hundred very ill of typhus and dysentery." "A considerable number of cases of flux and (typhus) fever, were received from the Gibraltar."†

At page 192, of vol. 1, of his *Medicina Nautica*, Trotter says, "There were a larger number of bad cases of typhus, ague, and dysentery, than usually come to a naval hospital at one time. From the same transport, and in the same regiment, were brought people of the three diseases just mentioned;" and in vol. 2, p. 14, he says, on board the Powerful, "a dysentery was associated with the fever."

It also appears in company with remittent and intermittent fevers, which are confessed by all to be produced by miasmatic exhalation, and is often converted into them, and *vice versa*.

There is an abundance of evidence in proof of this. Trotter says, "I have seen a family where the father was laboring under an obstinate tertian, while the mother and some of the children were ill in bed with typhus." "Nay, I have constantly remarked in those ships where contagion prevailed, that many cases of regular intermittents and remittents occasionally appeared. When they increased in proportion to the number of the continued type, and the latter becoming milder in its attack, I consider it as an infallible sign that the

* Pringle on Diseases, p. 25. † Trotter's *Med. Nautica*, vol. 1, p. 58.

power of the infection was on the decline, and would be speedily subdued.”*

“The attacks of fever,” says Trotter, “since leaving Cork, had been more numerous than on the preceding week. They were both of the continued and intermitting types.”†

About the beginning of March, the infection seemed to be on the decline; which appeared from the attacks being less severe; some cases of the remittent and intermittent type were now observed.‡

“The sick list consisted of seventeen venereal patients, and seven in fever, some of them regular tertians. Some were every morning discharged to duty, and others added in the course of the day, in both the continued and intermittent forms.”||

“Those cases (of typhus) which have occurred latterly, have been slight, with a general intermittent tendency.”§

“The Royal George did not seem to suffer much from the infection.” ¶ “They assumed a bilious remittent form.”¶¶

Trotter says, “It is not easy to trace those circumstances, which occasionally convert an intermittent or remittent fever into a continued type, (or typhus,) and *vice versa*. There must, however, be something in their disposition very much alike; for, wherever we find the typhus affecting a number of people at a time, we also find cases of the remittent and intermittent form.”***

Ferriar mentions one instance of the conversion of typhus into intermittent fever. “In one case,” says he, “where a typhus was unusually protracted, after several hazardous determinations to the stomach and bowels, the fever assumed the form of an intermittent.”††

Trotter, in speaking of the typhus on board the Valiant, says, “the cases were becoming milder; in some it put on the intermittent form, which showed that it was now on the decline.”‡‡

Dr. Armstrong, in a short essay on the origin, nature, and prevention of typhus fever, published in 1822, made a public recantation of the opinion he had long entertained relative to the contagion of typhus, and adopted that of its miasmatic origin, and its identity with intermittent and remittent fevers. The following account of the reasons of his change of opinion, is taken from the Medico-Chirurgical Review, September number, 1822. “Dr. Armstrong states, (says the Re-

* Trotter's Med. Naut. vol.1, p. 185. † Ibid, p. 311. ‡ Ibid, p. 187.

|| Ibid, p. 188. § Ibid, p. 70. ¶ Ibid, p. 83. ** Ibid, p. 311.

‡‡ Ferriar's Med. Histories, p. 82. †† Trotter's Med. Naut., vol.1, p. 66.

view,) that in 1819 he attended a patient laboring under an intermittent fever, which, in its progress, put on a remittent character, and ultimately a continued form and typhoid type, accompanied by very malignant symptoms. This case made a deep impression on his mind, and inclined him to ask, whether intermittent, remittent, and typhus fevers, might not possibly be modifications of one and the same disease. Up to this period, Dr. A. had firmly believed that human contagion was the sole cause of genuine typhus fever; but a doubt having been excited, he determined, if possible, to divest his mind of all former bias, and investigate the subject ab origine. In the course of the three succeeding years, a very great number of typhus cases have fallen under his notice, and the result of his observations is a decided conviction," "that what the Italians vaguely call *malaria*, and the English, as vaguely, *marsh effluvium*, is the primary source of typhus fever." In the next page the Review continues the subject, and says, "In tracing back many of the cases, Dr. A. found that they had commenced as intermittents; he has seen the remittent run into the continued typhus, and the continued typhus become remittent or intermittent."*

What stronger testimony can any one require to prove the identity of any two diseases, than the fact of their frequent conversion into each other? We have cited abundance of evidence to prove that this frequently takes place in typhus and acknowledged miasmatic diseases, and it is a fact, too, which cannot be evaded, unless by those who are absurd enough to contend, that a disease which they say is one *sui generis* and specific in its character, can change to one entirely different in its nature and origin. If there be any such persons, we will only ask them, if smallpox was ever known to change into measles, or *vice versa*, or cowpox into chickenpox?

It has been the common opinion for a long time, that typhus fever is a disease exclusively of the winter season, and hence the conclusion that it cannot arise from marsh effluvium, because the latter requires for its production, the co-operation of heat with moisture. We, however, deny the fact of its being a disease only of the winter season, and a

* This repudiation of a long cherished and favorite opinion is highly honorable to Dr. Armstrong; it shows a mind truly independent and devoted to truth, and clearly proves that he will not cling to an error, when convinced of its being such, merely for the sake of consistency of opinion. *Oh si sic omnes*. It, moreover, speaks volumes against the contagious nature of typhus. Dr. Armstrong never would have left the ranks where he had long fought as the most distinguished champion, without the most strong and irresistible reasons for his conversion.

reference to the different works on the subject will support us in the denial. Currie and Trotter mention many instances of its appearance and most extensive prevalence during the hot weather of summer and autumn, and Armstrong says he has known it to prevail during the greatest heats of summer. 'Tis true, that it prevails almost every winter, more or less, among the poor population of almost all the large cities both in England and this country; but, upon enquiry, it will be found, that those attacked have most generally been exposed to the sources of miasmatic exhalation during the preceding summer or autumn, and having thus acquired a predisposition to disease, the complicated and depressing miseries of poverty and want act as powerful exciting causes, and fever is produced.

We have now finished the subject of these pages; we cannot, however, say to our own satisfaction. We are conscious that much has been left unsaid, the ordinary limits of an essay not admitting of any thing like a wide range of discussion. We have endeavored to cull from the various works on the subject, the most striking and prominent facts bearing on the different subjects embraced in this essay. A considerable portion of it has been devoted to an examination of the different assumed causes of typhus, because we consider the possession of correct notions on this subject a matter of very great importance. Whether the conclusions we have arrived at are correct or not, must be left to the decision of others.

THE END.

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